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THE QUARTERLY JOURNAL OF
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AN EXPERIMENTAL CONTRIBUTION TO THE PROBLEM OF APPARENT REALITY

BY

MARGARET R. PHEMISTER

(From the Laboratory of Experimental Psychology, Louvain)

478K

Certain two-dimensional figures acquire a character of relief and reality when oriented obliquely to the observer and viewed monocularly. The experiments were made with two types of figure—a simple straight line, and more complex patterns of lines. The results indicate that the character of reality gradually increases as the experimental conditions become more favourable. The conditions found to be most favourable are dissimilarities of size, brightness, microstructure, shape, perspective clues, between figure and ground.

When the segregating factors are operating, the figure becomes independent of the paper and takes up its own orientation in space. In the case of the simple line, this orientation may be frontal, but more usually the orientation of the background is taken into account, the resulting structural organization being that of a "needle" projecting from the background. The reality is greatest when the orientation of the background is such that the "needle" appears to be perpendicular to the ground. The complex figures used in the experiments were perspective designs of parallelepipeds. Again, the oblique position of the design associated with the greatest degree of reality of the tri-dimensional object is that which permits the figure to be seen as projecting perpendicularly from its background.

Il y a certaines figures à deux dimensions qui se détachent d'un caractère de réalité quand on les observe en vision monoculaire et en position oblique. Pour les expériences on utilise deux espèces de dessin—une simple ligne droite, et des figures plus complexes. Les résultats montrent que le degré de réalité s'augmente en tant que les conditions expérimentales deviennent de plus en plus favorables. On trouve que les conditions les plus favorables sont des dissemblances de grandeur, de clarté, de microstructure, de forme, de perspective, entre la figure et le fond.

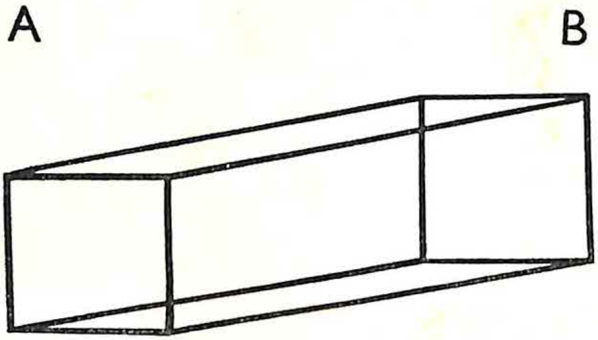
Les facteurs favorables donnés, la figure se détache du papier et se tient dans l'espace avec sa propre orientation. Pour la ligne droite, cette propre orientation peut être fronto-parallèle, mais plus souvent on tient compte de l'orientation du fond, et il en résulte que le champ s'organise comme une "aiguille" plantée sur le papier. Le maximum degré de réalité se produit quand l'orientation du fond est telle qu'elle parait être perpendiculaire à l'aiguille. On faisait aussi des expériences en employant des dessins de parallélépipèdes en perspective. On trouve encore le maximum degré de réalité quand l'orientation du dessin est telle qu'on le voit comme object tri-dimensionnel qui adhère perpendiculairement au papier.

I

INTRODUCTION

THE present work is the report of an experimental study concerning the problem of apparent reality in perception, which was raised recently by Michotte (1948) in an article on perspective in linear design. In this article, Michotte drew the distinction between reality and tridimensionality; tridimensionality is possessed by both solid objects and perspective drawings and is defined by the possibility of its equation with the volume of a solid object. The character of reality, on the other hand, is related to the possibility of manipulation, or rather, to the belief in the possibility of manipulation, of the form and is possessed by solid bodies but not by perspective drawings. However, it is possible for a design to acquire this character of reality under certain conditions, and Michotte gives a particular instance of this: an elongated perspective figure of a parallelepiped, if looked at monocularly and from a very oblique angle, becomes free from its background and looks like a real object, attached to the paper by one face only (*vide* Figure 1.)

FIGURE 1 (MICHOTTE *op. cit.*)



The figure should be looked at obliquely and monocularly, the eye being in a position almost in the same plane as the paper and preferably on the side B of the figure.

A simpler form of the same illusion was reported many years ago by Ladd Franklin (1887; 1896; quoted by Sanford, 1898): a number of short lines are drawn on a sheet of paper so that they all converge towards one point. Those lines are then looked at monocularly, the eye being placed in the position of the point of convergence of the lines and the sheet of paper being held almost horizontally on a level with the eyes; the lines them seem to detach from the paper and look instead like a group of short vertical sticks. The conditions of this phenomenon, as stated by Ladd Franklin, are as follows:

> "Lines which have any position whatsoever in planes passing through the axis of the body (or, for small near objects, in planes passing through the vertical meridian of the eye with which they are looked at) are taken by us to be vertical lines."

The converging lines throw their images on meridians of the eye very close to the vertical meridian and thus tend to be seen as vertical lines. The effect is reinforced by our experience that a large number of parallel lines are generally verticals, e.g. sides of buildings, trees, etc.

The same illusion arises when a single line is used; the line becomes free and appears to be *really* perpendicular to the paper. That is, it appears to have an independent existence and to be no longer merely a "trace" on the paper. The distinction between the "trace" character of the line and its appearance as something standing by itself and extending in space beyond the plane of the paper may be shown in a very simple way. If a simple V is drawn on a sheet of paper it can be seen as the perspective representation of a vertical stick standing perpendicularly on a plane which has as one of its features, a line which meets the base of the vertical stick. But if the V is looked at monocularly from an oblique angle, one arm of the V seems *really* to be detached from the paper and looks like a vertical stick, while the other arm remains in the plane of the paper.

It seemed that a systematic study of the illusions quoted by Ladd Franklin and Michotte would yield data relevant to the problem of apparent reality. This, then, is the purpose of the experiments reported in the following pages.

Ladd Franklin claimed that the condition which was necessary for the occurrence of the illusion was that of stimulation of the vertical meridian of the eye. The adequacy of this explanation seemed doubtful from the start, and the experiments were therefore devised with a view to discovering whether or not this was the only significant factor, and, if not, what were the other factors at work in this phenomenon. Two fundamental cases were studied:—

A that of a simple straight line;
B that of a more complex pattern.

II

SIMPLE STRAIGHT LINE

TECHNIQUE A.

The aim of the experimental procedure was to allow an observer to look monocularly at a given figure which could be presented in a variety of positions, more or less oblique to the frontal plane of the observer. The apparatus consisted of an optical bench, the arm of which was set parallel to the line of regard; card-frames which were rotatable about their vertical axes could be fixed at any given distance from the observer. The head of the observer was held steady in a head-and-chin rest, the height of which could be varied; by this means it was ensured that the eyes of each observer were on the same level, so that the horizontal meridian of the eye and the horizontal axis of the card-frame should be in the same horizontal plane. The general background of the apparatus was non-homogeneous—glass and woodwork of apparatus cases: in this way, the observer was better able to see the true orientation of the card frame than if the general background had been a homogeneous screen. During the experiments, the subject's left eye was covered; both eyes were closed between successive presentations. The presentations were made in a series which commenced with the frame set in a frontal position; the frame was then rotated clock-wise, so that the angle between the frame and the line of regard gradually decreased; the actual angles of presentation being 90° (frontal), 70°, 50°, 40°, 30°, 25°, 20°, 15°, 10°, 5°. For each position of the frame, the subject was asked to report on the appearance and location of the line.

Three subjects took part in this group of experiments. Each experiment was made only once with each observer, except in the case of the first observations that were made. These were carried out with lines of different lengths, these series being presented three times to each subject. Study of these results suggested that a fairly clear general picture could be obtained by presenting only one series to each subject.

Discussion of the Group A Experiments.

The first experiments were made with a horizontal black line of length 6·6 cm. and width 1 mm. which was placed centrally on a square white card of side 20 cm. From these first experiments certain general conclusions could be drawn which were supported by all the later experiments.

Firstly, it is clear that the impressions which arise depend on the angle between the plane of the card and the line of regard. At the more acute values of this angle, the line appears independent of its background and much shorter than it appeared in the frontal position; as is well known, size constancy holds only within a limited range. Change of impression is gradual; when the card is presented in the frontal plane the line is clearly seen to be designed on the paper, but by 70° it begins to free itself from its background, and this freedom gradually increases until by 30° there is usually complete freedom, though sometimes this occurs as early as 50°.

Secondly, it is necessary to distinguish three different types of structural organization which may arise:

(*a*) The line sticks to the paper and is seen in the same perspective as the whole frame, but there is practically no constancy of length, and phenomenally the line becomes gradually shorter as the angle of presentation decreases.

(*b*) The line achieves a greater or less degree of freedom from the paper. Even in the frontal plane, although it appears to be *on* the paper, it does not strictly belong to the paper. In the case of our technique, using black lines on plain white paper, it is particularly noticeable that there is in the line no trace of the microstructure which is apparent in the surrounding paper. As the angle of presentation decreases, the distal end of the line becomes more and more free of the ground, while the freedom of the proximal end increases to a considerably less extent. Consequently, the line is often seen as curved or broken. At the most acute angles, the independence of the line increases to such an extent that the line touches the paper only at its proximal tip, and it is seen as a "needle" sticking into, or sticking through, the background. The angular relation between the "needle" and the paper is at first, usually, somewhat acute but gradually increases towards perpendicularity for the smallest angles of presentation.

(*c*) The line achieves complete freedom from the ground; it appears to be located in the frontal plane and quite unrelated to the paper. The location and appearance of the ground are not very clear; sometimes it also seems to be frontal, of trapezoid form, but still separated from the line; more often it has the appearance of a "fog" or film colour with no defined limits.

Thirdly, it seems that the experimental conditions are ambiguous, inasmuch as identical physical conditions may give rise to these differently structured impressions, either on different occasions or during a single prolonged observation, when there may be succession or alternation of two or more types of organization. It is possible, also, that different observers may favour different types of structure. For instance, Subject B perceived almost exclusively the first two types of structural organization, rarely seeing the line as if it were located in the frontal plane. Subject K, on the other hand, seemed to favour the frontal structure, perceiving this organization at an earlier stage of the series than he could perceive the "needle" organization.

Other factors influencing the type of structure perceived on any given occasion include: the structure which dominated in the immediately previous presentation; the types of structure seen in previous experiments; more specifically, an attempt to estimate the length of the line results in the favouring of the type (*c*) (frontal) organization.

As a consequence of the gradual change of impression, the diversity of possible structures, and the ambiguity of the responses, it is difficult to formulate any definite rules as to the precise conditions corresponding to the emergence of an impression of relief and reality. But already, these experiments, by utilizing a horizontal line instead of a vertical line, have disproved Ladd Franklin's claim that verticality is a necessary condition.

In order to ascertain whether or not vertical lines have any privilege over lines which throw their images on the horizontal meridian of the eye, the apparatus was changed in such a way that the same card (with the 6·6 cm. black line) was presented so that the line was vertical and the card was rotated about the horizontal axis instead of the vertical axis. The same series of angles of presentation and the same conditions of observation were employed. The results from these series were almost identical with the results from the horizontal line. As a further control, two more cards bearing straight black lines of 15 cm. and 3 cm. were presented so that the lines were vertical and rotated about a horizontal axis. The results from these were again similar to the results obtained when the same cards were presented so that the lines were horizontal and rotated about a vertical axis.

A second change in the apparatus enabled us to present the frame in any oblique position, with the axis of rotation also oblique, instead of either vertical or horizontal. Again the figure used was the 6·6 cm. black line on the 20 cm. square white card. Initially the frame was set so that the line was horizontal; then the frame was tilted sideways about the sagittal axis so that the axis of rotation which had been vertical was now oblique. The frame was then rotated about this axis in the usual manner and as in the previous experiments, the line frees and stands out perpendicularly from the card for the smaller angles of regard. The strength of the impression of reality is in no way less than that obtained when the presentation was made about a horizontal or a vertical axis of rotation. In fact, if the acute angle between the card and the line of regard is maintained and the whole frame is rotated round the sagittal axis, the observer perceives no change in the quality of the impression, but merely sees an oblique white plane, out of which projects a short black needle, the whole thing being rotated in space. It is clear that throughout this experiment the line threw its image on some meridian of the eye and that this meridian was always perpendicular to the meridian which was parallel to the axis of rotation of the card.

These experiments provide further evidence in support of our criticism of the theory of Ladd Franklin, since it seems that exactly the same results are obtained whatever the position of the line or the plane, provided that the plane is rotated about an axis perpendicular to the line.

All the following experiments are classified into six groups, each of which throws light on a specific factor which it seemed might have some influence on the phenomenon.

1. To start this more detailed study, it was decided to investigate the possible influence of having *the line oriented on the card in such a way that it was not perpendicular to the axis of rotation*, or to impose fixation conditions which made it impossible for the subject to see the line as perpendicular to the axis of rotation. For the first part of the investigation, the material consisted of a set of cards, 20 cm. square with straight black lines of length 6·6 cm. and width 1 mm. designed on them at angles of 5°, 10°, 15°, 20°, 30°, 40°, 50°, 60°, 70° to the horizontal, in such a way that the proximal end of the line always occupied the same point of the card. The centre of the card was 40 cm. from the right eye of the subject. The procedure and apparatus used was that described in Technique A. Though no point of fixation was imposed, it is probable that the observers viewed the line in such a way that its image fell on one of the oblique meridians of the eye, and so it was impossible for the subject to see the line as projecting perpendicularly from the background which was rotated round a vertical axis.

The results from this series of experiments show that oblique lines also achieve a certain degree of freedom from their background, but that this freedom is not so pronounced as in the case of the horizontal line. The strength of the impression of freedom appears to decrease as the angle between the line and the horizontal increases and disappears altogether for small angles of presentation when the projection of the line on the retina is almost vertical, i.e. almost coincident with the axis of rotation, the line then appearing as a vertical trace on the vertical card.

Some individual differences are noticeable, making it therefore impossible to indicate clearly defined limiting positions associated with the different impressions. As previously noted, Subject B shows distinct preference for the "needle" type of structure. And, of the three subjects, M perceives most readily the true perspective structure of the field, while B most easily sees the line freed from its background as a real object. But for all three subjects the type of change in impression was similar as the conditions varied; for instance, for two presentations such that M perceives the line, in one case, as designed on the paper and, in the other case, as a needle with a small degree of reality, B is able to perceive the former as an unreal needle and clearly sees the second as a very real needle. In other words, the trend of the changes is the same, but it would be possible to give definite quantitative measures of the conditions corresponding to the different types of impression, only if large numbers of subjects were used and the results treated statistically.

The second part of this investigation was to arrange the conditions so that the stimulus line did not throw its image on a meridian of the eye, and so could not be seen perpendicular to the axis of rotation of the card. A horizontal black line, length 6·6 cm. and width 1 mm., on a square white card of size 20 cm. was presented to the subject four times in succession: the first time, the observer was asked to fixate the proximal end of the line; the second time, to fixate a point 1 cm. below this end of the line; the third time, to fixate a point 2 cm. below the end of the line; the fourth time, to fixate a point 3 cm. below the end of the line.

The responses showed that as the image of the line became farther removed from the meridian of the eye, the more difficult it became to see the line projecting from the paper. Fixation of the tip of the line favoured the freeing of the line, and gave the "needle" structure of the field preference over the "frontal" structure. For all the series where the point of fixation was below the line, the impressions were not altogether clear, even for the frontal presentation of the card. From the point of fixation nearest the line, the line was seen as a needle for the more acute angles of presentation, but not as easily in this series as when the line itself was fixated. From the point of fixation 2 cm. below the line, the line was seen as a needle only for one or two of the most oblique positions of the frame, and this organization was not very stable. The "needle" organization was not achieved at all when the lowest point was fixated. The lack of sharpness of the line in these latter cases made it difficult to define any relation between the line and the paper and so the line was often seen in a frontal plane.

Summarizing the results of these experiments, we know that a line will detach from its background and take on a stable character of reality only if its retinal image falls on, or near, a meridian of the eye. We also know that the character of reality of a line is very weak if the line is not drawn perpendicular to the axis of rotation of the ground. Since we have already shown that the illusion can arise whatever meridian of the eye is stimulated, the absence of the illusion in this case cannot be attributed to the absolute orientation in space of the line; hence, we may conclude that it is the condition of perpendicularity between line and plane which is significant. To illustrate the truth of this, the apparatus was changed so that the frame could be rotated about all three orthogonal axes. The material used was the set of cards bearing lines which were oblique with reference to the coordinate axes of the cards. Here again it appeared that by adjusting the position of the plane with respect to three axes, it was possible to find an orientation of the plane which enabled the observer to have a strong impression of reality from the line. When this orientation of the plane was achieved, it was always found that the relationship between the "needle" and the plane was that of perpendicularity.

2. The next step was to discover what influence the *length of the line and the size of the ground* might have on the illusion. For the first observations, the size of the card was kept constant, 20 cm. square, the breadth of the black lines was kept constant, 1 mm., but the lengths of the lines were varied. Three different lengths of horizontal line were used— 3 cm., 6·6 cm., 15 cm. Following this, observations were made using smaller cards, again white, 10 cm. × 6 cm., with lines of width 1 mm., and lengths 3 cm. and 6·6 cm. The cards were presented so that the lines remained always on a level with the horizontal meridian of the eye and the method of presentation was that described on p. 5. The vertical axis of rotation of the frame was 100 cm. from the eye of the observer.

From the responses to these experiments it appears quite clearly that a short line frees more readily from its background than does a longer line. But that this is a question of relative length rather than absolute length is shown by the fact that lines of equal length on different-sized backgrounds tend to stick more readily to the smaller ground; hence the line of 6·6 cm. shows less freedom from the small card than from the large card; and this is also true of the line of 3 cm. although its degree of freedom from the card still remains greater than that of the 6·6 cm. line.

It seems, too, that at our observation distance, the smaller cards are more easily seen in their true orientation than the larger cards, which readily take on an indefinite, foggy appearance and therefore lend themselves rather to the "frontal" type of organization than to the "needle" type. Thus the absolute size of the card may affect the position taken up by the line when it is free, but it is the relative sizes of the line and the ground which determine whether or not the line shall become free in the first place.

To complete this section of the investigation, an experiment was made with a white card, 20 cm. square, which had a line drawn horizontally across it, from edge to edge, representing the limiting case of equality of size of line and ground. The results from this experiment were exactly what one would expect from such conditions—there was absolutely no tendency to freedom of the line, which remained completely integrated with the card for all positions of presentation and appeared as a dividing line belonging to the contour.

3. The possible variation of impression with *change in distance* between figure and observer was the next problem to be examined. The cards used here were the three large cards, 20 cm., bearing the lines of lengths 3 cm., 6·6 cm., and 15 cm. These cards were presented at a distance of 40 cm. from the subject, and the responses compared with the responses given when the same cards were presented at 100 cm. from the subject. The most striking difference in the results is the greater ease with which the lines are seen in perspective when the cards are presented nearer the subject. This is achieved, at least in part, as a consequence of the difference in sharpness of the ends of the lines, the difference in sharpness being shared by the two edges of the card. Probably at short distances, since the similarity of perspective clues from the line and the card is then more apparent than for greater distances, this similarity goes to unify the figure and the ground.

4. A necessary condition for the illusion is the use of *monocular vision* and so this must be listed as one of the significant factors at work in the phenomenon. In binocular vision, the presence of binocular parallax which acts in the same way for both the line and the card, results inevitably in their being seen as a unit, a line traced on the card.

5. The material for the next set of experiments was devised with a view to discovering what influence the factor of *brightness difference* would have upon the phenomenon under investigation. The material consisted of three white cards, 10 cm. × 6 cm., one having a pale orange line, 3 cm. × 3 mm., designed on it; another having a darker orange line of the same size; the third having a black line of the same size. The figures were presented at a distance of 100 cm. from the observer.

The lack of ambiguity or individual differences in these results makes it quite clear that the greater the similarity of brightness of figure and ground, the more strongly unified the two remain; and it is only when there is a very marked difference in brightness, that the line achieves total freedom and is seen as projecting from the ground, for very acute angles of presentation.

6. During the first experiments it was remarked that even when the figure was presented in the frontal plane, it had a certain amount of independence, and we thought that this might be due to the fact that there was no trace of the microstructure of the ground apparent in the figure. To determine whether or not this difference in *surface appearance* was an influential factor, it was necessary to discover whether or not the illusion still occurred when the microstructure of the ground and figure was the same. For this purpose, small transparent coloured rectangles were designed on cards, 10 cm. × 6 cm., of mottled grey paper and of white paper of very rough surface. These irregularities of surface appearance were visible through the transparent figures. Control cards of the same material but with heavy black rectangles of the same size, through which the microstructure of the ground was not visible, were also used. These figures were presented at a distance of 100 cm.

The results from these series of observations were very striking, in good lighting, when the microstructure was clearly visible, the transparent figures were seen persistently as coloured patches on the surface of the paper; while the opaque black figures achieved a fairly strong degree of freedom. These results may be partly attributable to differences in brightness; but that they are in large measure due to the differences in microstructure is shown by the fact that in weaker illumination, when the microstructure is not clearly visible, the figures showed some tendency to freedom.

III

General Conclusion to Group A Experiments

These experiments have made it clear that there are a number of factors which may be favourable or unfavourable for the freeing of a drawing from its background. Any factors which help to unify the figure and the ground, similarity factors, prevent the figure from becoming independent. Factors which help to segregate the figure and ground favour the perception of the figure as something standing on its own, with the appearance of reality.

All the factors can be listed in such a way as to show how they can operate favourably for the phenomenon:

Orientation of the card in such a way that the line may be seen as perpendicular to the background.

Marked difference in size between figure and ground so that there is no tendency for the line to form a whole with the contour of the card.

- Lack of similarity of perspective clues from figure and ground—in particular, the perspective clues which arise when there is not a very great distance between figure and observer.
- Use of monocular vision.
- Strong brightness contrast between figure and ground.
- Strong microstructure contrast between figure and ground.

If the converse factors are operating, the figure belongs entirely to the ground.

In conclusion, we may say that as long as there are integrating factors present, the line belongs to the card, or sticks to the card, and is seen as being drawn on it. On the other hand, when segregation factors are at work, the line is freed from the card. And, this being so, we might expect, *a priori*, that the line would always appear in the frontal plane, independent of the card, since the line always appears in the frontal plane when it is seen through a reduction screen. This is what actually happens in structure (*c*) (p. 4); but, generally, since there is no reduction screen, the card and its orientation in space are clearly seen and it is therefore seen as a real object tilted in space and not as a film colour. Consequently, we are having to do with a more complex structural organization including both line and card. Under these conditions, our experiments have shown that the maximum freedom of the line is given when the line seems to be *perpendicular* to the card. It seems, then, that this perpendicular structure may be considered as a pregnant structure or pattern. And this is shown by the fact that in such cases, the line does not appear to lie in the frontal plane (as it does in structure (*c*)) but instead its position is determined by the perpendicularity as such. There are, then, two possible "freedom" structures—total independence, or integrated structure of line and background which possesses most freedom when it has the favoured perpendicular form.

To show that the perpendicular organization of the field was immediate, a tachistoscopic experiment was made. The field presented in the tachistoscope was a white card, oriented obliquely to the line of regard, the card bearing simply a short black line. The point of fixation corresponded to the position of the proximal end of the line. In tachistoscopic presentation to four subjects, this field was organized immediately in the structure of an oblique white card with a short black pin projecting from it. Thus the freedom of the figure from the ground seems to be immediately given, independently of eye-movements. A control experiment, using a card with a real pin projecting from it, showed that the observers perceived no difference between the card bearing the line and the card bearing the real pin. The subjects for this experiment had all taken part in previous experiments and so these results might be influenced by training; this should be controlled.

IV

Experiments B. Complex Patterns

The use of a complex pattern immediately introduces a change in the quality of the perception: many such figures can be seen even binocularly as perspective drawings, and certain ones, such as cubes, parallelepipeds etc., have a special perspective value. So that these complex figures have a stronger potentiality of reality than a simple straight line. We may start, however, with the simplest cases. Reference has already been made to the case of the V which is seen, under conditions of monocular vision and acuteness of angle of regard, as a vertical stick standing on an oblique plane which has a line designed on it which meets the base of the stick. This "stick" has much more reality than has a single line seen under the same conditions. The strong impression produced by the V is demonstrated by slowly turning the paper bearing the V, still looking at it monocularly and obliquely, so that the arm which first appears as a stick gradually sinks into the plane of the paper and the other gradually rises out of the paper and becomes the stick. The presence of the second arm of the V, by its dependence on the paper, seems to accentuate the independence of the first arm. A further reason for the stronger impression of reality aroused by a complex pattern is probably to be found in the fact that a complex of lines helps to give more structure to the ground as well as to the figure; and hence it helps to define the relation between the figure and the ground. And, as it has become clear from the previous group of experiments that the angular relation between the figure and the ground is of great importance, any conditions, such as complexity of design, which help to emphasize any such favourable relationship will necessarily enhance the character of reality of such a figure.

This was shown strikingly in an experiment using a white card, 20 cm. square, with a black horizontal line, 6·6 cm. long and 1 mm. wide, drawn on it and crossed at its proximal end by a short, thick, black vertical line. The card was presented to the subjects according to Technique A (p. 3) and the differences between the responses to this figure and the responses to the simple line of the same length on a card of the same size, are very marked. The horizontal line of the T-shaped figure was seen as projecting from its base, with a strong degree of reality, and never seen as a completely detached line in the frontal plane with no defined relation to the background, as often occurred with the plain line.

A further series of experiments was made with a variety of more or less complex patterns. The figures presented were of two types: simple contour figures whose outline was ± 1 mm. thick; and similar contour figures enclosing a straight horizontal line of length 6·6 cm. and width 1 mm.; these figures were designed on white cards, 20 cm. square, and were presented at a distance of 100 cm. from the subject. The technique of presentation was the same as that of the Group A experiments (p. 3). The simple contour figures included: square; rectangle with long horizontal axis; cross with long horizontal arm. The figures enclosing the straight line were: narrow rectangle; figure with straight sides and curved ends—two of these were used, one being very narrow, and the other being less narrow so that the sides of the figure were farther removed from the enclosed line.

The results from these experiments indicate that a figure whose perspective follows that of the card is seen most easily in the plane of the card. Not only is it true that such a figure is seen more easily in the plane of the card, but if it encloses a simple straight line sufficiently closely for the figure and the line to form a unit, the line also is held to the plane of the background. Exceptions to this general rule are found when the frontal form of the figure, i.e. the stimulus form, is as pregnant as, or more pregnant than, the perspective form. This question of pregnancy will be shown later to be of major significance.

Some experiments were also made with simple parallel lines, but the results were fairly inconsistent among the different observers and several very different structural organizations were produced.

The final experiments were made with true perspective figures. Two slightly different designs were used, but the method of producing them was the same. A real wire parallelepiped of square cross-section 5 cm. and length 8 cm. (thickness of wires, ± 1·5 mm.) was set perpendicularly against a white vertical background, 40 cm. square, so that the posterior square face of the model was flat against the background and the sides of the model were horizontal and vertical. This parallelepiped was then photographed, but in order to have a good perspective image, the camera was placed sideways towards the right and at a higher level than the parallelepiped. For the first photograph, the distance from the centre of the model to the lens of the camera was 87 cm.; the "horizontal" angle (i.e. the angle between the horizontal projection of the optical axis of the camera and the plane of the background) being 71° 20′, and the "vertical" angle (i.e. the angle between the optical axis of the camera and the vertical axis of the background) being 74° 15′. A second slide was made under the conditions: distance from lens to centre of model, 81 cm.; "horizontal" angle, 69°; "vertical" angle, 81°. Several identical copies of these exact perspectives were made on photographic paper by projecting the slides on to a screen under the same conditions of distance, vertical angle and horizontal angle; so that the image thrown on the screen was the exact perspective representation of the real figure and on the same scale as the real figure.

TECHNIQUE B (1).

Two of the identical designs made from the first slide were used in this first set of experiments. One was fixed in the "correct" position, at 87 cm. from the eye of the observer, at a "vertical" angle of 74° 15′, and at a "horizontal" angle of 71° 20′, so that the image thrown on the retina by this design should be exactly the same as would have been thrown by the real parallelepiped if the eye of the observer were in the position of the objective of the camera. The other design was fixed on a frame whose centre was 87 cm. from the eye of the observer and on the same level: the observer was able to change the position of the frame which could rotate about its vertical and horizontal axes. The subject, whose head was fixed in a head-rest, was required to compare the two figures and vary the position of the frame so that the difference in reality between the two figures should be maximum. After this first adjustment was made, the subject was asked to alter the position of the frame again in order to find some other orientation which gave an equally strong impression of reality. These double adjustments were made five times by four subjects: all the subjects agreed that the *impression of reality* evoked by the exact design as seen from the exact point of view was considerably less strong than the impression of reality called out by the same design seen from a different point of view, and the required adjustments were made without any difficulty. It should, however, be noted that when the design was adjusted to the position of greatest reality, the apparent shape of the parallelepiped was quite different from that of the control figure.

Results and Discussion of Experiment B (1).

In the course of the adjustments of the position of the frame, there was relatively small range of variation in the tilting about either axis. The initial frontal position of the frame is taken as being represented by angular values, about both axes, of 90°. If the results are divided into groups of roughly comparable sizes and the averages of these groups are taken, the frequencies of the adjustments for the different couples of vertical and horizontal angles are:

Vertical angle (i.e. *tilt about horizontal axis*)		*Horizontal angle* (i.e. *tilt about vertical axis*)		*Frequency*
Average	*Range of group*	*Average*	*Range of group*	
54·5°	—	50°	—	1
48°	51°–45°	58°	65°–53°	8
41°	44°–39°	52°	58°–45°	10
37°	38°–34°	46°	55°–44°	6
31°	33°–29°	45°	53°–37°	7
24°	28°–21°	44°	51°–48°	6

(two subjects made only a single adjustment at the first presentation bringing the total number of adjustments to 38 instead of 40).

It is to be noted that these angles are not *immediately* comparable with the positional angles of the control figure in this experiment, because of the difference of position of the two drawings relative to the eye, but the results do indicate that certain combinations of vertical and horizontal angles evoked impressions of reality much more frequently than did other combinations; and, more particularly, that the greater the horizontal angle, the greater must also be the vertical angle to produce a strong impression of reality; and vice versa.

Taking the averages of the first, preferred, adjustments made by the subjects during the five series of observations, we have—

Subject P ..	Av. vertical angle	45°	Av. horizontal angle	50°
Subject M ..	,,	43°	,,	55°
Subject K ..	,,	39°	,,	50°
Subject B ..	,,	30°	,,	42°

The figures for the first three subjects suggest that there seems to be an optimum position for the appearance of reality of the design, and that this position is certainly very different from that corresponding to the real perspective. The results from Subject B were not altogether consistent with the results of the other three subjects, but it was discovered that the form of figure appearing most real to this subject was different from that preferred by the other subjects. This will be discussed later. At this stage of the experiments it was sufficient to note three general points.

1. A given design of a parallelepiped evokes a less strong impression of reality when it is set in its correct position, corresponding to the exact position of the real parallelepiped from which the design was made, than when it is set in some other position where there is a more acute angle between the plane of the design and the line of the regard. In other words, a drawing does not have its greatest degree of reality when it stands in a position exactly corresponding to its perspective.
2. Positions in which the design arouses strong impressions of reality may be achieved by changing the angle between the plane of the design and the vertical, and the horizontal angle between the plane of the design and the frontal plane of the observer.
3. For any such design, there is probably an optimum position where its appearance of reality is strongest.

Further experiments were therefore planned to derive more detailed information relative to these two last general conclusions.

TECHNIQUE B (2).

The same apparatus and the same design were used for this set of experiments as had been used for the previous set. Only the procedure was varied. The experimenter set the frame at a certain vertical angle (thus the frame was tilted forwards) and the subject, looking monocularly, was asked to adjust the horizontal angle of the frame (i.e. to turn the frame around its vertical axis) so that the character of reality of the figure should be maximum. Ten random series of adjustments for seven different vertical angles were made for the same four subjects who had participated in the preceding experiment. The subjects were also asked to describe the form of the figure when it seemed most real.

Results and Discussion of Experiment B (2).

The results for the three subjects M, K, and P followed the trend indicated in the preceding experiment; as the vertical angle of the frame was increased, the horizontal angle was also increased to give the most satisfactory impression of reality. The average values of the adjustments of these three subjects were as follows:

	Horizontal angle		
Vertical angle	*Subject M*	*Subject K*	*Subject P*
55°	56·1°	51·1°	58·2°
50°	55·3°	51·6°	58·8°
45°	53·1°	52·6°	57·2°
40°	51·6°	49·8°	54·4°
35°	48·4°	50·6°	54·2°
30°	46·9°	49·2°	52·2°
25°	47·2°	47·9°	48·6°

On the other hand, the results of Subject B showed an opposite tendency; as the vertical angle was increased, the horizontal angle was decreased.

Vertical angle	*Horizontal angles: Subject B*
55°	35·5°
50°	34·7°
45°	37·2°
40°	41·4°
35°	43·1°
30°	44·2°
25°	48·6°

This inconsistency in the results becomes intelligible when the descriptive results are taken into account. For the three subjects M, K, and P, the figure seemed most real when it appeared to be projecting perpendicularly from the background; for Subject B, the figure was most real when the oblique lines of the figure were hanging down vertically from the background: that is, for Subject B, the form most real was that in which the projecting lines lay in fronto-parallel positions; for subjects M, K, P, the form most real was that in which the projecting lines were perpendicular to the posterior face of the figure and therefore not frontal with respect to the observer.

It seems, then, that there are two possible favourite positions in which a figure may possess a maximum character of reality. The existence of such favoured positions implies a functional relationship between the vertical and horizontal positional angles of the figure, relative to the observer; the actual relationship between the two being different for the two types of organization. For what is probably the more usual type of organization, the relationship appears to be in terms of one angle being an increasing function of the other, and this corresponds to apparent perpendicularity of the parallelepiped on its background; in the other case, the one angle seems to be a decreasing function of the other.

TECHNIQUE B (3).

The other tentative conclusion which was drawn from the preliminary experiments in this group and which demanded verification, was that there is an optimum position for such a design to give the strongest impression of reality. A method for testing this conclusion was devised and the material employed consisted of five of the identical designs which had been used in the two previous experiments. These five designs were fixed, one below the other so that the distances between their centres were constant, at 19 cm.,on a wooden frame which could rotate about its vertical axis. The observer's head was held in a head-rest in such a way that the distance from his right eye to the centre of the top design was 87 cm. and the angle between his line of regard and the vertical axis of the frame was 74° 15′; so that when the frame was rotated to the horizontal angle of 71° 20′ (90° representing as usual the frontal position of the plane) the top design was in the "correct" position, according to its perspective.

Before the experiments were made, each subject was shown a specimen drawing to indicate what was meant by apparent reality; most of the subjects had never performed such experiments and so it was necessary to start with a short demonstration to show what was to be observed and none of the experiments were started until the subjects fully realized the difference between a perspective drawing and a drawing possessing apparent reality.

The procedure of the experiment was as follows: the frame was set in a fairly oblique position and the observer was asked to look monocularly at the five designs and say which one (or ones) seemed the most real. The frame was then set at a different horizontal angle and the subject again required to judge the comparative reality of the five parallelepipeds. Five series of observations were made with each of thirteen subjects, each series consisting in the presentation in random order of the figures in the positions corresponding to the horizontal angles 71° 20′, 60°, 50°, 40°, 35°, 30°, 25°, 20°. At the end of the five series, the subject was asked to look at the top design only, set in the control position of 71° 20′ and answer the following three questions:

What is the shape of the figure?
What is the orientation of this figure with reference to the background?
What is the orientation of the posterior face of the figure with reference to the background?

The frame was then set in an oblique position and the subject asked to look only at the design which, according to his previous judgments, had the strongest appearance of reality, and the last two questions, referring to the orientation of the figure itself and of its posterior face, were repeated.

Results and Discussion of Experiment B (3).

The percentage results in favour of each design are given in the following table. The designs are numbered 1–5 according to their position on the frame, No. 1 being the top one and No. 5 the lowest one; associated with their positional number is the "vertical" angle between the line of regard, directed at the centre of the appropriate design, and the vertical axis of the frame. If two or more designs were considered equally real, both preferences were noted: hence the number of judgments for any position of the frame may exceed the total number of series of observations (65). If all the designs were seen as equally real, or unreal, this response was classed as Doubtful (?).

	Design number	1	2	3	4	5	?	*Total number of judgments*
Horiz. angle	*Vertical angle*	74° 15′	63° 6′	53° 43′	46° 9′	40° 6′		
71° 20′		25·3	34·1	11	8·8	9·9	11	91
60°		14·1	31·8	24·7	15·3	1·2	2·3	85
50°		14·5	20·5	30·1	22·9	10·9	1·2	83
40°		7·5	10·8	30·1	36·6	15·1	—	93
35°		7·5	4·3	29	41·9	16·1	1·1	93
30°		9·2	3·9	19·7	**52·6**	14·5	—	76
25°		6·7	2·2	16·8	51·7	21·3	1·1	89
20°		7·9	5·7	13·9	45·5	27·3	—	88

The trend of the results is similar to that of the other experiments, showing that there is some kind of functional relationship between the vertical and horizontal angles of presentation. But it is also apparent that there is an optimum position for this design, i.e. at a horizontal angle of about 30° and a vertical angle of about 46°. These quantitative results are borne out by the descriptive results; although most of the observers could pick out, for any angle, one or two designs as being more real than the others, the designs which were picked out as the most real at the large horizontal angles did not look as real as those picked out as being most real at the more oblique angles of presentation. Even those subjects who found difficulty in distinguishing different degrees of reality between the designs at any given horizontal angle, agreed that they had more reality for the more oblique positions of the frame.

The responses to the questions regarding the shape and orientation of the figures were very consistent. For the design in its "correct" position, all the subjects saw the figure as a cube or parallelepiped, oriented obliquely to the background, but in almost all cases the posterior face itself was seen to be in the same plane as the background. Occasionally, however, this face was seen to be oblique to the ground in the same way as the figure as a whole. But the greater number of subjects gave the apparently contradictory responses which imply that the posterior face of the figure is situated in two different planes at the same time. When the questions were repeated in reference to the design when it was in the position associated with the strongest impression of reality, the figure was seen to be perpendicular to the background by all but one of the subjects. Once more, the form of the figure was no longer that of an elongated parallelepiped, or even a cube, but had the shape of a very flat parallelepiped.

Following this, the five designs were replaced by five other identical designs made from the second of the photographic slides (the angular values 69° and 81°); exactly the same apparatus and procedure were used with five subjects and the percentage results, shown in the table below, completely support the results of the former investigation. The functional relationship between vertical and horizontal angles is again apparent and the existence of an optimum position is clear, though this position is evidently different from the optimum position for the first design.

	Design number	1	2	3	4	5	?	*Total number of judgments*
Horiz. angle	*Vertical angle*	74° 15′	63° 6′	53° 43′	46° 9′	40° 6′		
71° 20′		53·1	31·2	6·2	—	—	9	32
60°		59·4	34·4	6·2	—	—	—	32
50°		47·1	41·2	11·8	—	—	—	34
40°		27·6	65·5	6·9	—	—	—	29
35°		20·7	75·8	3·4	—	—	—	29
30°		13·3	80	6·7	—	—	—	30
25°		10	83·3	6·7	—	—	—	30
20°		—	**100**	—	—	—	—	25

The angles (71° 20′ and 74° 15′) which corresponded to the exact perspective of the first design did not correspond to the exact perspective of the second design, but were used in order to have exactly the same experimental conditions.

V

General Conclusion

The most important conclusion to be drawn from the Group B experiments is that a simple perspective drawing is not most real when it is designed in such a way as to be perspectively correct from the point of view of the observer; though it is perhaps in such a case that there is the greatest degree of likeness between the shape seen in the picture and the shape of the represented real object. This is certainly true for ordinary pictures, generally speaking. But it is not under these conditions that one has the strongest impression of apparent reality of extension in the third dimension. The results of the experiments also indicate the probable reason why this is so, and consequently the conditions under which a given design will acquire the character of reality. There is a conflict in the perception of a perspective drawing when it is looked at from a normal point of view. In the particular figure we have been using, the conflict arises because the parallelepiped is normally seen as oriented obliquely to the background; i.e. the oblique lines of the figure do not seem to be perpendicular to the background. But these lines are seen to be perpendicular to the posterior face of the figure. Yet the figure is so placed on the paper that this posterior face appears to be located in the plane of the paper and if this were true then the oblique lines should seem to be perpendicular to the paper, which is evidently not true. The conditions, then, are such that the figure cannot be satisfactorily seen as a real form oriented either obliquely or perpendicularly on the background. The conflict arising from these two opposing organizations is resolved by the loss of reality of the figure which is therefore seen as a two-dimensional representation of a solid object.

Therefore one would suppose that if the objective conditions were arranged in such a way that no such conflict would arise, then the figure would have a compelling appearance of reality. In other words, the figure would become real if the observer saw the oblique lines of the parallelepiped as perpendicular to both the posterior face of the figure and to the plane of the paper. And this is the case when the drawing is looked at monocularly from an oblique point of view, as has been shown by our experiments; but in that case, there is a distortion of the apparent shape of the object. In fact, in our experiments, the parallelepiped looked much flatter when it seemed most real than it did when seen as a perspective drawing in the frontal plane.

For a few subjects, the design has always a certain degree of reality even when it is shown in the control position. In these cases, the posterior face of the figure is seen as being oriented obliquely to the background in the same way as the whole figure—in other words, the freedom of the figure from the ground is so strong for these subjects that the usual conflict does not arise and consequently there is no loss of reality. But even for these subjects, the impression of reality is stronger when all the objective conditions are tending to produce the same perceptual organization. It is perhaps possible to train subjects in this direction, to obtain segregation between figure and ground, even when looking at the picture from the exact point of view.

As to the reason why the obliquity results in apparent reality of the third dimension, the two sets of experiments give relevant information. Results have shown that there is a preliminary condition—namely, that integrating factors, tending to bind the drawing to the plane of the paper, should be reduced to a minimum, and that segrating factors should be strengthened. We have seen that the integrating factors were mostly factors of similarity, i.e.:—

1. Similarity of size of figure and size of ground.
2. Similarity of perspective clues arising when the distance between subject and figure is small.
3. Binocular parallax.
4. Similarity of brightness of figure and ground.
5. Similarity of microstructure of figure and ground.
6. Similarity of perspective distortion of design and support.

When these integrating factors are reduced, there is a possibility of the figure becoming segregated from the plane. But to actually achieve the segregation and the organization in the third dimension, our experiments have shown that another factor must operate. In our experimental conditions, the organization in the third dimension becomes more real when the orientations of the lines and the background are such that the object may seem to extend *perpendicularly* from the surface of the ground. This seems to be a pregnant form of organization which arises as soon as the pattern of stimulation allows it.

Throughout this report, we have been speaking of the freeing of the figure in a purely introspective way. A stroboscopic experiment was therefore devised to demonstrate the freeing in a more evident manner; the demonstration was found to be quite coercive, the conditions being such that all the relevant factors were co-operating to produce the same impression. Of the two figures used in the stroboscope, the first consisted of a 3·2 cm. horizontal black line, width 1 mm., whose proximal end was enclosed by a red contour square of 1 cm. side; the centre of the square was not exactly at the tip of the line, but was displaced 2 mm. along it, so that when the figure was looked at from an angle of about 15°, the tip of the line appeared to be located at the centre of the square. The second figure consisted of a short black line of length 8 mm. and width 1 mm., drawn so that its tip was at the centre of a red contour square of 1 cm. side. Both figures were drawn on similar white cards, 10 cm. × 6 cm. As these cards were set in the stroboscope, the observer saw alternately the first figure at an angle of about 15° (between the line of regard and the plane of the card) and the second figure in a frontal position (90°). Hence, for each of these views, the central black line had the same stimulus length of 8 mm. while the size and orientation of the red square and white card changed. The apparatus was set so that the observer, looking monocularly, was presented with the two views in rapid succession in such a way that the image thrown on the retina by the black lines remained exactly the same. This resulted in the compelling impression that the white card and red square base turned from a frontal position to a very oblique position while the black line retained its independence and orientation in the frontal plane, and was linked with the plane only at the point where it touched it in the centre of the red square. To produce this compelling impression it is essential that all the factors are working in the same sense; for instance, it was found that there was a certain amount of ambiguity in the impression if the line and the square were both drawn in black. This similarity in brightness and colour of the two parts, one of which tends to be free and the other of which tends to remain in the background, completely destroys the illusion for some observers.

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LEARNING AND UNDERSTANDING

BY

M. D. VERNON

(From the Department of Psychology, Reading)

Short texts containing rather difficult pieces of logical argument were presented, sometimes with illustrative graphs and sometimes without, to girls from the sixth form of a grammar school. They were required to study these, and recall afterwards what they were about. It was found that frequently there was a comparatively accurate recall of many of the individual items of the texts, but that certain essential points in the argument were omitted. Thus learning of detail could occur when there was no general understanding of the meaning. The effect of the illustrative graphs was to make the recalls even less coherent.

I

INTRODUCTION

To conclude investigations* of the ability to understand information, presented graphically, it was decided to discover if information was derived more readily from study of a brief written text illustrated by graphs, or from a written text without such illustration. In the course of this inquiry, there appeared some rather startling evidence of the inability to understand the meaning of the material presented. In this paper are discussed some of the results of this inability, and the conclusions which may be drawn with regard to the effect of learning by rote upon learning by understanding of meaning.

II

METHOD OF EXPERIMENT

The material used in the experiment consisted of two short written texts, each of about 400 words. The first dealt with recent changes in the sizes of populations, and the relation of these changes to fluctuations in the birth and death rates. The second outlined the relation of disease and death to poverty. Graphs were prepared which showed the main data upon which the arguments of the written texts were based. There were five graphs for each text, and they were interleaved with the text so that each graph appeared on the page on which was presented the corresponding verbal information.

There were two groups each of 11 subjects, who were girls aged 16–18 from the sixth form of a grammar school. The first group was given Text I illustrated by graphs and Text II without graphs; and the second group was given Text I without graphs and Text II with graphs. The subjects were tested individually. Each one studied Text I for ten minutes, and then endeavoured to recall as much of it as possible, orally; the recall was recorded verbatim. Text II was then treated in the same way.

The recalled material was scored in two ways: First, one mark was given for each individual item of information remembered (only items of some significance were scored—linking sentences were omitted). But there were also certain general statements or groups of items the omission of which showed that some essential part of the argument had not been fully grasped and remembered. There were seven of these key points in each text; and the number of these omitted was scored separately.

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* An earlier investigation into the understanding of material presented graphically was carried out by the author while a member of the scientific staff of the Medical Research Council. A brief account of this is given in the *British Journal of Psychology*, 1946, **36**, 146–158. A report on a further investigation will be published shortly in the *British Journal of Educational Psychology*.

III

RESULTS

1. *Effects of Difficulty of the Material.*

As in previous investigations, it was found that the material of Text I was considerably harder to understand than that of Text II. The number of individual items recalled was 45 per cent. of the total for Text I, and 46 per cent. for Text II; the difference was not significant ($P = 0{\cdot}78$). However, 59 per cent. of the general statements of Text I were omitted, and 30 per cent. of those of Text II; the difference was highly significant ($P < {\cdot}01$).

When the recalls were examined for qualitative differences, the effect was even more apparent. The original of Text II consisted mainly of a succession of fairly straightforward statements such as, that adult and infant mortality were higher in the lower than in the upper social classes; that certain diseases showed this effect particularly clearly; and that certain conditions of livelihood inherent in poverty appeared to be responsible for these effects. These statements could be separately assimilated, and if they were not recalled in exactly the right order, the general gist of the passage was not greatly impaired.

In Text I, on the other hand, the logical succession and inter-relations of the statements presented in the text was much more marked. The most important statements were that the population of Great Britain had increased up till the present day, whereas the birth rate between 1880 and 1940 had steadily decreased. These at first sight contradictory effects were reconciled by the stated fact that increase of population occurs as long as the birth rate exceeds the death rate, which it had done and still is doing. Now one subject only gave this essential relating statement in her recall. Thus it was apparent that the other subjects did not really grasp the general argument; and indeed, their recalls were full of inconsistencies and errors. The most frequent mistake was a confusion between size of population and birth rate. Thus it was said in one place that the birth rate of Great Britain had decreased, and in another place that it had increased (whereas it was the population that had increased). And it may be, of course, that the difficulty in grasping the essential point noted above was due to the fact that size of population and birth rate were never clearly differentiated by the subjects.

Whatever the source of the difficulty, the fact remained that the omission of a single key point prevented the subjects from really understanding the general argument of the passage as a whole. What appeared to remain in their minds was a jumble of ideas about population, with few if any clear ideas of what was happening to it, and why. But we noted above that the percentage of individual items recalled was not significantly less in Text I than in Text II. Hence it appears that an individual is capable of reproducing quite a number of isolated facts, but they may be jumbled up, without any obvious inter-relationship, and without leading to any coherent meaning.

This is not to say that even the individual items remained intact in the general confusion. A further consequence of the lack of understanding of the general meaning of the passage was that words and phrases were reproduced often with only slight changes from the original; but the changes were such that they might destroy the true meaning of the phrases, as in the following examples:—

> "There was at one time an increase in birth rate, but only very little over the death rate because there were more people dying." "The birth rate slowly increased owing to people living longer." "In 1940 the birth rate increased slightly, so that there was a small increase of population but only slight; so the population may again be stationary and decrease." "In 1880–1941 the birth rate seemed higher because the number of births had been comparatively more than the death rate of the people each year."

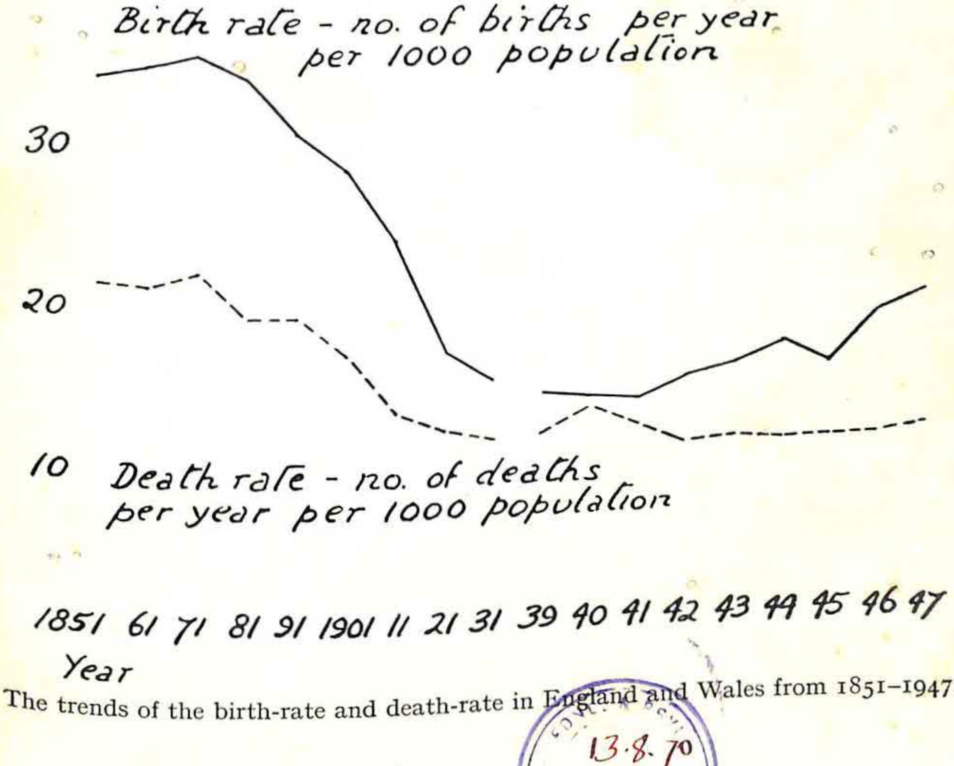
Again, when certain important items were omitted in the spontaneous recall, they could often be elicited from the subject by asking appropriate questions. In other words, this information had been retained by the subject, but it was unattached to the remainder of the information by a coherent scheme of relationship. Therefore it was not spontaneously available. Thus a subject cannot make efficient use of information which cannot be "fitted in" to some general argument.

2. *Effects of Graphical Illustration.*

Before passing to a more general discussion of the implications of these results, a few comments may be made on the effect of the graphs. In general, they seemed to decrease the accuracy and coherence of the recall. The number of individual items recalled was 37 per cent. of the total, when there were graphs; and 54 per cent. of the total when there were no graphs. The difference was significant ($P < \cdot 01$). In the

FIGURE I

GRAPH ILLUSTRATING TEXT I.



The trends of the birth-rate and death-rate in England and Wales from 1851–1947.

13.8.70
9167

first case, 51 per cent. of the more general and essential statements were omitted, and in the second case, 38 per cent., the difference being significant ($P = 0{\cdot}048$).

From a general examination of the recalls it appeared that the subjects lacked the ability to integrate the specific facts presented in the graphs with the statements made in the text. Sometimes the graphs were ignored completely, after a cursory inspection. Sometimes the subject spent so long and became so confused in trying to incorporate facts from the graphs that essential points from the text were omitted. And sometimes a description of the graph would be suddenly introduced in the wrong place, or produced as an afterthought at the end of the recall. Thus one subject, having completed a recall of Text I which more or less followed the text, added at the end:—

> "The death rate decreased till 1939, then shot up during 1940–41, but has been decreasing since then. The birth rate decreased till 1941, rose slightly, then it's been decreasing gradually" (see Figure 1).

In general, the recalls of material from the written text alone were better arranged, smoother and more coherent than those of the text with graphs. In certain circumstances this tendency of the graphs to break up the continuity of the written text might serve the useful purpose of preventing the slavish acceptance and copying of the statements in the text. But this effect would be valuable only if the subject were able, first to assimilate the details of the graphs; secondly, to check the written statements against them; and thirdly, to integrate the whole into a coherent logical argument. It did not seem that the subjects of this experiment, at least in the time at their disposal, were able to perform this task.

IV

CONCLUSIONS

We reach then the conclusion that more or less accurate memorization of particular words and phrases, and even of whole statements, can be achieved without any assimilation of the general argument or understanding of the ultimate meaning of a written passage. Indeed, it appears almost as if the effort to memorize detail accurately may preclude a more general understanding. But this seems a contradiction of the "effort after meaning" tendency. What is more probable is that if the meaning can be obtained only with considerable effort, and if the subject lacks the ability and the interest to make this effort, he is liable to fall back on the recall of isolated items, as if to save a few remnants from the general confusion.

Certain consequences arise from this. In the first place, we see that partially assimilated information may be a ready source of inaccurate rumours and tendentious opinions. Thus, there was a sentence in Text II which stated: "Tuberculosis, which is particularly fatal to young women, has a close relationship to wages." This was reproduced more than once as: "Tuberculosis is fatal to young women." It so happened that these girls were about to undergo mass radiography. The implications are obvious! We can understand how quite sensible statements in a newspaper, when isolated from a meaningful context, may be distorted by the displacement of a few words to produce wholly erroneous impressions.

Secondly, it seems useless to test a subject's real understanding and recollection of a connected prose passage by scoring the number of isolated items remembered. Such items will be useless as they stand, and are indeed unlikely to persist in their original form.

Lastly, these results throw considerable doubt upon quantitative theories of learning. The efficient learning of a meaningful task may be almost in inverse proportion to the number of individual items learnt, in particular, of isolated words and phrases. For if the subjects could coherently reformulate in their own words the general arguments of the text, one could be fairly certain that the arguments had been grasped. If, on the other hand, the words of the text were slavishly copied, one would be much more doubtful. Accurate reproduction of detail is not necessarily inconsistent with memorization of a general argument, but it does not ensure it, and may, indeed, make it more difficult.

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THE DIRECTION OF MOVEMENT OF MACHINE CONTROLS

BY

M. J. H. MITCHELL and M. A. VINCE

(From the Medical Research Council Applied Psychology Unit, Cambridge)

Experiments are described in which the relation between the direction of movement of a control and display was varied in order to determine what relationships are likely to cause least confusion to the operator of a machine. Two different types of task were used: a task consisting of intermittent stimuli whose rate of presentation could be varied, and a continuous pursuit task.

It is found that accuracy of performance varies with the degree of remoteness of the directional relationship from those met in everyday life, with the complexity of the task, with the ability of the subject, and there is some indication that under certain conditions it may vary with the breadth of his attention. Awareness of the response aspect of the situation in these simple sensori-motor tasks, it is suggested, although necessary in the early stages of learning, may be associated in the later stages with confusion and error.

I

INTRODUCTION

In many mechanical skills met with in industry and everyday life, the operator of a machine is required to produce movements of one part of the machine (the "display") by making directional movements with a control, such as a knob, lever or handwheel. One simple aspect of the problem of machine design is thus concerned with the question of what relationships between the direction of movement of the control and that of the display are most quickly and effectively learned. It is generally recognized that an "expected" directional relationship—one in which the display and control move in the same direction—will be the easiest to learn, and will be least likely to cause error or accidents. The "expected" relationship, however, may not always be immediately practicable for the designer, and in this case it is important to know whether an "unexpected" relationship, in which display and control move in opposite directions, or an ambiguous relationship, will cause more confusion to the operator of the machine.

Many experiments have been concerned with this problem, and the stage seems to have been reached where general conclusions can be drawn from them. For this purpose a number of these experiments have been summarized below. The results appear to indicate that an untrained operator is likely to show real preferences for certain directional relationships and, moreover, that under certain conditions preferred directional relationships are reflected in performance, in a serial task. The effect of an "unexpected" or ambiguous relationship may not be felt, however, unless the task is one where the required response rate is fairly high, or the task itself fairly complex, or unfamiliar as when performed by the left hand. It is suggested that the confusion resulting under these conditions may be produced by forcing the operator to direct his attention to the response aspect of the situation, as awareness of the response appears to be associated with difficulty and error.

It is not known whether intensive training will remove the hampering effect of an "unexpected" relationship in a serial task, but such a task seems to be difficult to learn under certain conditions, especially for subjects with low intellectual ability.

II

Preferred Directional Relationships

In the handling of machine controls it is known that certain directional relationships will be "expected," or regarded as "natural" by the majority of operators, while other relationships may be "unexpected" or seem "unnatural." There is, in addition, some experimental evidence to show what these preferred relationships are when subjects are merely asked to move a control in what they think will be the appropriate direction to bring about a specified movement of a display. Warrick (reported by Fitts 1947) has shown that when rotary movements of a control knob are required, and where indicator and control move in the same plane, the operator usually moves the control so that the part of it nearest the indicator moves in the direction in which he is attempting to move the indicator, but where the control knob does not move in the same plane as the indicator the response tendencies are ambiguous. These results have been confirmed by Gibbs (1950) in the operation of actual machine tools using a handwheel.

Similarly, using levers and pointers moving at right angles to each other and considering each hand separately, Mitchell (1948) found that a large majority of right-handed subjects favoured movements inwards to move the pointer up (right hand to the left or clockwise, and left hand to the right or anti-clockwise), and the opposite movements, outwards (right hand to the left, and left hand to the right) to move the pointer down.

These results, which show a general meaningful trend, although they do not give a complete picture, are summarized in Table I.

TABLE I

Preferred Directional Relationships

	Display moves up and down, control on right-hand side of display: *% giving clockwise movement for up and vice versa*	*Display moves up and down, control on left-hand side of display:* *% giving anticlockwise movement for up and vice versa*	*Display moves to right and left.* *% giving clockwise movement for right and vice versa*	*Display moves backwards and forwards, control with axis parallel to operator.* *% giving clockwise movement for towards operator and vice versa*	*Display moves backwards and forwards, control with axis at right-angles to operator.* *% giving clockwise movement for away from operator and vice versa*
Warrick (knob) (Fitts 1947).	60	57	77	27	30
Gibbs (hand-wheel) (1950).	89·3	—	93	43	46
Mitchell (lever) (1948).	78 (right hand)	78 (left hand)	—	—	—

Note: Warrick's figures generally give lower percentages as his group contained a number of subjects who were inconsistent. Gibbs and Mitchell gave subjects only one try. Warrick's figures are probably the most reliable therefore, but the same trend is shown in all.

III

THE EFFECT OF DIRECTIONAL RELATIONSHIP ON PERFORMANCE

a. Discontinuous Serial Tasks.

In addition to the evidence of preference for certain directional relationships, there is evidence that under certain circumstances some combinations of direction that are "unexpected" when incorporated into a serial task, give rise to an initially poorer performance than other, "expected" relationships.

There are obvious reasons for this; it can be demonstrated that if a stimulus (e.g. an indicator that requires to be moved *up*) calls for a response that is not the one that would be the first to be thought of (an upward control movement) the time lag between stimulus and response will be longer than if an "expected" response movement were required. It has, further, been shown (Vince 1950) that the reaction time for an "unexpected" response even after a long period of practice is longer than that for an "expected" response. The reaction times in this case were 0·419 compared with 0·359 second. Performance which requires a high response rate should therefore be affected adversely by an "unexpected" control-display relationship.

An experiment showing that this is the case, is described by Vince (1945). In this experiment (which will subsequently be referred to as the "discontinuous task") the stimuli were separate ink marks either above or below a horizontal line on a moving band of paper, and subjects responded to each mark by moving a control knob in the "expected," or same, or in the "unexpected," or opposite, direction. The knob had an all-or-none effect, and actuated a pencil marker, so that subjects had immediate knowledge of results. Results show that when stimuli are separated by an interval of four seconds the difference between "expected" and "unexpected" connections is slight, that when the interstimulus interval is two seconds, subjects made about twice as many errors with the "unexpected" as with the "expected" connections, and that at faster rates, the difference between the two becomes even greater; what happens is that performance with the "expected" connection shows little deterioration up to an interstimulus interval of 0·5 second, and then falls off, while with the "unexpected" connection, performance falls off at intervals shorter than one second, and then more rapidly.

A further question which arises from the problem is whether more apparently ambiguous relationships, where preferences are less unanimous, are easier, or more difficult to grasp and learn than the more obviously "expected" and "unexpected" relationships. Using a similar task, but with stimuli spaced irregularly, and appearing at a mean rate of 1·4 per second, Vince and Mitchell (1946) were able to place six directional relationships in an order of accuracy of response, as shown in Table II. Over the initial learning period which was studied, this order seemed fairly stable, and in it directional relationships in different planes were placed between the "expected" and "unexpected" relationships.

The stability of these results was tested by giving the subjects an extra task to carry out with the left hand (Mitchell 1947). Here the previous experiment was repeated, but at the same time subjects operated a handwheel at a constant rate, winding anti-clockwise, with the left hand. The effect on the right-hand task seems to have been to exaggerate the confusion caused by difficult relationships, while preserving, in the main, the rank order of accuracy.

Here again, the rank order of accuracy, and scores, obtained in this experiment are given in Table II. In both experiments using the discontinuous task, the differences in error score between the various directional relationships were mostly large enough to be statistically significant.

TABLE II

EFFECT OF DISPLAY-CONTROL RELATIONSHIP ON PERFORMANCE

(Results from Vince and Mitchell 1946, Mitchell 1947, 1948 and 1949)

Discontinuous task		*Continuous task*			
				Right hand in 2-handed task:	
One hand	*Two hands*	*Left hand only*	*Right hand only*	*Left hand = F*	*Right hand = B*
Error score—mean of 5 runs	*Error score—mean of 5 runs*	*4th Run error score*	*4th Run error score*	*Error score*	*Error score*
U 2·98	U 4·16	U 2·51	L 2·67	F 1·91	U 1·89
F 4·52	F 5·3	R 2·73	U 2·86	U 1·93	L 2·03
R 6·6	— —	B 3·04	R 3·04	L 2·31	D 2·13
B 7·0	L 9·2	F 3·32	B 3·14	R 2·51	B 2·53
L 7·4	D 11·12	D 3·32	F 3·25	D 2·73	R 2·67
D 7·98	B 11·88	L 3·79	D 3·54	B 3·72	F 2·79

"U" = control movement up to move pointer up, and down for down.
"F" = ” ” forward ” ” up, ” backward ”
"R" = ” ” right ” ” up, ” left ”
"B" = ” ” backward ” up, ” forward ”
"L" = ” ” left ” ” up, ” right ”
"D" = ” ” down ” ” up, ” up ”

The effect of directional relationship on performance appears to be fairly marked in the discontinuous task, and the resulting rank order relatively stable. With the continuous task differences are very slight when the right hand is used, and a little more marked when the left hand is used; here again there is some suggestion of a similar rank order. In the two-handed task the rank order might be considered as produced by the combined effect of the practised left hand relationship and the "naturalness" or otherwise of the right hand relationship. As these results are taken from six different groups of subjects, *inter* group comparisons are likely to be misleading.

b. Continuous Pursuit Tasks.

A discontinuous serial task was chosen for the experiments previously described, as it was the simplest that could be envisaged for this investigation and could be varied in a controlled way for speed and directional relationship. As a task, however, it is not particularly related to the conditions of ordinary life, where rapid series of discrete movements are not often required. For this reason the results obtained previously were checked using a simple pursuit task. The chief differences between this and the preceding task being that now (i) the stimulus is changing continuously, so that for certain periods of time, the rate and direction of movement can be anticipated, also (ii) that knowledge of results is forced on the subject, as the misalignment depends on his previous performance as well as the cam-controlled pointer movements, and (iii) that the response rate is determined by the operator, although the size of misalignment that he has to correct depends on the rate at which the stimulus is moving as well as on the accuracy of his immediately preceding performance. In this experiment the task for the subject was to keep a moving pointer on a horizontal line, by moving a handle up or down. The pointer was moving continuously up or down, at the fairly slow speed of approximately 0·4 in. per second. As before, the connection between handle and pointer could be varied in the same six different ways.

Under these conditions, when right-handed subjects performed the task with the right hand there were no statistically significant differences between error scores obtained with the six different directional relationships used, although there were small differences giving a rank order of accuracy which is given in Table II with the scores (Mitchell 1948). This confirmed the finding of Grether (1947) that, when a pointer with an irregular oscillating motion had to be maintained over a reference mark by means of a rudder pedal or stick control, directions of control and display had little effect on the accuracy with which the unpractised subject performed a short one-dimensional tracking task. That the directional relationship is of little importance in a simple one-dimensional tracking task was shown by Vince (1945). Here it was found that when the relationship between control and display was reversed between two trials without warning, half of the subjects continued to track as accurately as before and did not notice the change. Whereas the other half did notice and their performance deteriorated, but this feature of the results will be discussed later in Section 6.

It seems then that in a simple continuous pursuit task where the preferred hand is being used, the direction of movement of the control in relation to that of the display is not an important element of the performance, and that conscious awareness of this relation is not necessary for efficient tracking.

A tentative explanation of this difference between the effects of the directional relationship on discontinuous and continuous tasks has been found in the operator's tendency to oscillate in the continuous task; in the early stages of learning he may correct a misalignment by overshooting the target, this corrective movement being followed by a second overshoot in the opposite direction. Successful tracking could therefore be carried on by the trial and error method of making alternate movements in opposite directions. This method would require little cognitive appreciation of the connection between control and display. An alternative method was to lag behind the target and put in a series of corrective movements in the same direction, until the direction of the target motion itself changed. Again, when errors of direction did occur they could be rapidly appreciated and corrected before a large misalignment developed, and might not always be detectable in the records, although efforts were made to assess them.

It is possible, however, that a more fundamental explanation might be found in a factor which forms an integral part of this continuous task and is less important in the other: that of knowledge of results. As stated above, the fact that in this task a misalignment is the algebraic sum of the target course and the operator's response presents the subject with knowledge of results in a particularly emphatic form: it is not only what he has done, but what he has to do. It is this factor which makes it possible to call the continuous task a skill according to the definition of Bartlett (1947b); it has pre-eminently the "character of being in touch with demands which come from the outside world." Faced continually with the information of how well he is responding to these demands relieves the subject perhaps of the necessity of turning his attention to the response.

A one-dimensional tracking task differs then, from the discontinuous task in that the directional relationship is not an important factor in performance.

A slightly different picture emerges, however, when tracking results for the left and non-preferred hand are considered (Mitchell 1948). Here an order of accuracy emerged, and is given in Table II, and here some of the differences (those between the best and two worst, and the worst and two best relationships) are statistically significant. It is of interest to note that where handle movements towards the right and left are concerned both for right and left hands the best performances were obtained with those relationships which were found to be the preferred ones (see Section 3), although this did not hold in the discontinuous task where differences between performance with right and left response movements for an upward movement of the display were slight.

Again, when a tracking task is performed with both hands simultaneously, statistically significant differences appear between different directional relationships (Mitchell 1949). Here subjects watched two pointers, each of which was moved independently by a cam, and also by a handle, operated by the subject, one for each hand. Two different conditions were employed. The two groups of subjects were given the task in six sessions, in each of which the directional relationship for the right hand was different, while that for the left hand remained constant throughout, being for the first group a movement of the control handle forwards, to move the pointer up, and for the second group the left hand task was reversed, so that a backward movement of the control was required to move the left hand pointer up throughout. The results given in Table II suggest that the rank orders of accuracy obtained in this way were affected by the most familiar aspect of the situation; the directional relationship employed by the left hand. This seems to have dominated the mental set of the subjects and rendered more difficult otherwise "easy" relationships for the right hand. It is, for example, interesting to note that whereas when the left hand relationship was a forwards movement to move the pointer up, that relationship was also the best for the right hand while the opposite relationship, a backwards movement to move the pointer up, was the worst and this was significantly different from all the others. (Also in this experiment the two best, and two worst relationships were significantly different from each other.) Again, when the left hand relationship was reversed, for the second group to a backward control movement to move the pointer up, the greatest amount of error was made when the relationship for the right hand was a forwards movement to bring the pointer up. However, in this part of the experiment the backwards movement for the right hand occupies its approximately normal position in the rank order, and the only difference which was statistically significant was that between the best and worst relationships.

The main point brought out by this experiment is that when the situation is made more complex, performance again appears to be affected by the directional relationship between control and display.

Similarly, in a rather complicated display-control relationship problem which seems to have contained some elements similar to both the continuous and discontinuous tasks, Carter and Murray (reported by Fitts, 1947) found that the relationship between the axis of movement of the control and the line of movement of the display was important and also that the relationship between the direction of movement of the display and of the control was important for reducing errors and time for performance. In their words:—

> "It would seem that, if the display and response required are very simple and if considerable time is allowed for responding, the display-control relationship may not be important, but if the display or response required is complex or if the operator is hurried, the display-control relationships are of extreme importance."

Some further evidence which points to the same kind of conclusion is provided by Craik (1944), where constant rates of handle winding had to be maintained by right and left hands separately. When the rate of winding was slow (10 r.p.m.), right and left handed performances were approximately similar, but when the rate of winding was increased to 100 r.p.m. larger winding errors were made by the left hand than the right, although these differences were not statistically significant.

A continuous pursuit task where the response consisted of rotary movements of a handwheel is also described by Gibbs (1950) and he found that there was a considerable difference between initial performance and also time required to learn to a certain criterion of accuracy between the "expected" and "unexpected" relationships. To obtain a perfect record his subjects would have had to wind at a rate of 18 r.p.m., so that their required rate of movement was approximately 2·8 in. per second. Here also the directional relationships were more complex as a clockwise pointer motion had to be balanced by clockwise or anticlockwise handwheel movements, and the task more complex as subjects had to superimpose corrective movements on this high basic winding rate, which must itself need adjustment from time to time. There are, then, two essential differences between these winding tasks and the continuous task of Vince and Mitchell described above: complexity of directional relationship and speed of the course to be followed. It is shown in the experiments mentioned above that an increase in the complexity of the task will result in a difference between the "expected" and "unexpected" relationships even in this continuous task, and it is possible that a considerable increase in speed might have had the same effect. The latter, however, is an hypothesis which remains to be tested.

IV

Learning and Individual Differences

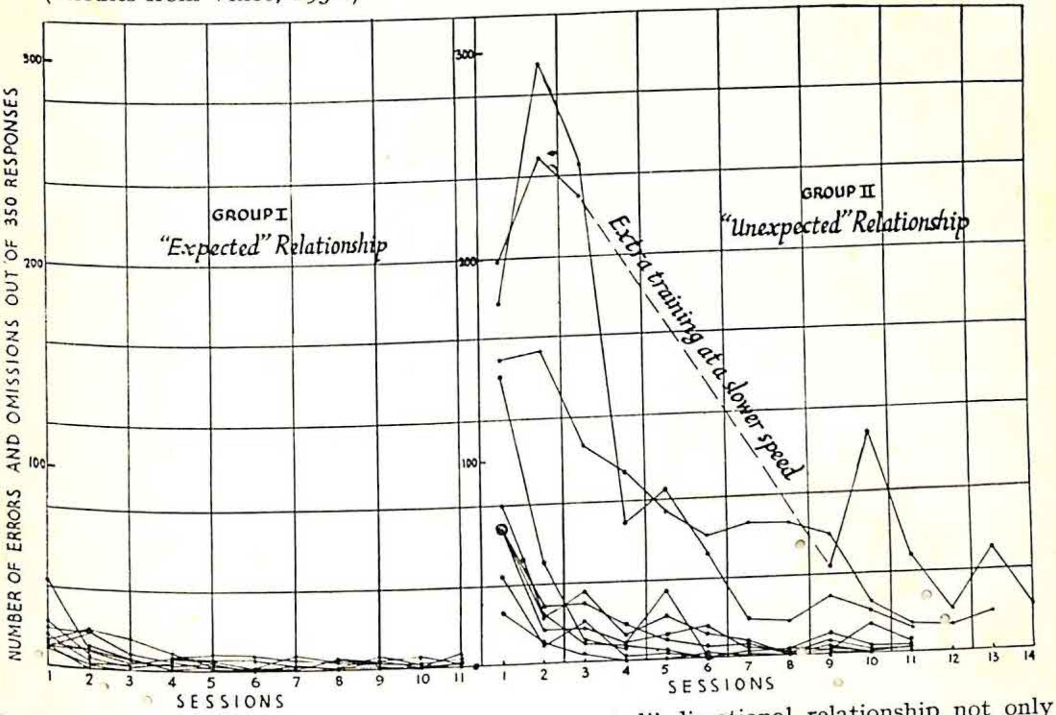
An attempt was made by Vince (1950) to compare rates of learning, and also retention under disturbed test conditions with the "expected" and "unexpected" directional relationships, in a discontinuous task where subjects were not given visual knowledge of results. There were two matched groups of subjects. The "unexpected" group on the average achieved a lower level of performance at the end of the learning period, than the "expected" group. Their retention of the skill under disturbed conditions was also inferior to the "expected" group. There is also a suggestion in this experiment that, in the same way as increasing the complexity of the task exaggerates the difference between performance with the different directional relationships, so these more difficult tasks also exaggerate the differences between subjects, on the basis of their initial performance and rate of learning. It was pointed out by Vince (1945) that although the difference between errors made with the "expected" and "unexpected" discontinuous tasks was statistically significant, nevertheless, there was much overlap between scores obtained on these two tasks: the "expected" connection affected a few subjects to a marked degree and most very little. The same kind of picture emerged in the learning experiment. Here differences between subjects who carried out the task using the "expected" connection were slight, but the other group of subjects doing the same task with the display-control relationship reversed, varied considerably both as regards their initial performance and their rate of learning (Figure 1). These individual differences were in the main preserved throughout the learning period, and there was a suggestion that ability to perform this "unexpected" task correlates with intelligence, as might be expected, considering that the cognitive element in such a task is relatively dominant.

FIGURE 1

LEARNING AND INDIVIDUAL DIFFERENCES WITH "EXPECTED" AND "UNEXPECTED" DIRECTIONAL RELATIONSHIPS.

INDIVIDUAL LEARNING CURVES.

(Results from Vince, 1950.)



Individual learning curves show that an "unexpected" directional relationship not only affects initial performance and rate of learning as compared with an "expected" relationship, but differentiates between subjects to a marked degree. In this experiment subjects were not given immediate visual knowledge of results.

V

ROLE OF AWARENESS OF THE RESPONSE

The preceding results suggest that the differences between performance with various display-control relationships are associated with the amount of conscious understanding which is required for their performance, or the degree to which the relationship is removed from those practised in daily life.

In this type of task there is, of course, a high conscious appreciation of the *end* for which the task is performed; the subject is concerned with what he has to do and how successfully he is doing it. But the preceding experiments tend to indicate that where the task is less familiar and more complex the *response* itself, as well as the result of the response must be included in the subject's field of attention.

In order to investigate this problem to some extent a number of experiments were devised where the connection between control and display was reversed in the course of the experiment, thus reversing the direction of the required hand movements. This was done mostly between two trials, and always without warning the subject. It was discovered that in many cases this reversal could be made without being noticed by the subject, and sometimes without affecting his performance to any marked degree. Results were assessed by dividing the subjects into three groups, those who noticed the reversal, those who did not notice, and those who were

TABLE III

Effect on Performance of an Unexpected Reversal of Directional Relationship

(Results from Vince (1945) and Mitchell (1946))

1. Discontinuous Task

(1) Fast rate *Directional Relationship*			*Uncertain*	*i. Before reversal*				*ii. After reversal*			
				Noticed		*Did not notice*		*Noticed*		*Did not notice*	
Group	*Before reversal*	*After reversal*	*Number of subjects*	*Score*	*No. of sub-jects*	*Score*	*No. of sub-jects*	*Score*	*No. of sub-jects*	*Score*	*No. of sub-jects*
(*a*)	Forwards	Backwards	3	4·3	15	4·6	7	8·2	15	6·1	7
(*b*)	Backwards	Forwards	6	6·6	10	8·0	9	8·4	10	8·6	9
(*c*)	Right or Left	Left or Right	13	5·4	19	8·1	18	7·6	19	5·6	18
	(2) *Slow rate*										
(*d*)	Up	Down	—	0·8	5	—	—	6·8	5	—	—
(*e*)	Down	Up	—	0·4	5	—	—	4·8	5	—	—

2. Continuous Task

Directional relationship		*Mean error score*											
		3 Trials: before reversal						*3 Trials: after reversal*					
Before reversal	*After reversal*	*Noticed*			*Did not notice*			*Noticed*			*Did not notice*		
		1	2	3	1	2	3	4	5	6	4	5	6
Up for up	Down for up	2·25	1·65	1·77	2·43	1·65	1·79	7·14	2·83	2·1	1·86	1·82	1·47

These results show that on the whole, subjects who noticed that the directional relationship had been reversed, tended to deteriorate more after the reversal than did those subjects who did not notice. When stimuli were presented at a slow rate all the subjects noticed the reversal. With the discontinuous task the different levels of achievement attained with different directional relationships confuse the picture. For example, for group (*a*) the first part of their task, *before* the reversal was intrinsically easier than the second half, while the reverse was true for group (*b*).

uncertain, or who noticed *something*, but could not specify what change had taken place. These results are summarized in Table III. For each combination of directions chosen the mean error score is given for each group of subjects both before and after the reversal, and the number of subjects falling into each group is also given. These data are taken from Vince (1945) and Mitchell (1946).

As stated earlier, when the task was a continuous one a large proportion of the subjects did not notice the change, and these as a group put up a better performance after the reversal than did the group who did notice; as long as the subject did not notice the change, the relationship between direction of movement of control and display could in fact be reversed without greatly affecting the operator's performance.

In the discontinuous task, subjects always noticed the reversal except when the stimulus rate was as fast as two stimuli per second. (In this experiment stimuli were equally spaced, and the reversal was made between stimuli rather than between trials.) Results from a further experiment at a faster rate, and where the reversal again took place between two trials are given in Table III. Here the records are less easy to assess for the different levels of performance achieved with the different directional relationships confuse the picture. In the main, however, it seems that those who noticed the change (and these were in general the subjects who worked according to a "system," i.e. who had consciously worked out the directional relationship when first tackling the task) tended to put up a better performance before the change, and their performance deteriorated more afterwards, than did the group who did not notice. These differences are however slight, and statistically reliable only in one case. But here, again, it was the subjects who did not notice it who were least affected by the reversal. The conclusion to be drawn from this part of the experiments seems to be that for the discontinuous task a conscious working out of the relationship helped in the early stages of learning (this does not seem to have been necessary in the continuous task), but it could be a drawback in the later stages.

This view of the role of awareness, or the inclusion in awareness of the response, or the direction of the attention towards the response, and a coupling of awareness with a deterioration in performance, is in agreement with the statements of previous writers on the nature of skill (see Bartlett, 1943, and 1947a) and provides a possible clue to differences between these continuous and discontinuous tasks as a whole and a possible explanation of some experimental results outlined above. It seems that when a stimulus requiring a directional response is perceived the subject brings to the task a preformed mental set which enables him to make a response in the same direction as the stimulus, with a minimum of cognitive interference (due to his previous experience). But if the required response is not in the same direction as the stimulus, either the wrong response will be made, or the whole task must be approached with a new mental set. The new mental set will presumably require an awareness of the total situation. But if the wrong response is made, subsequent events may depend on the nature of the task. The correct readjustment may be made almost automatically in a moderately easy task and where the operator has his attention directed towards the stimulus, or the error may, in a difficult task, or with a subject who tends to think of the response, again reproduce the early stages of a difficult learning situation, where sensory and motor aspects of the situation, which have no obvious connection, must be harmonized.

The main conclusion which can be drawn from these results is that as long as the response is efficient, i.e. satisfactorily adapted to the situation, it can be carried on without the attention being directed towards it, but as soon as errors are made, or for some reason the task becomes difficult, the attention is directed towards the response and it may be disrupted further. Cause and effect are not possible to distinguish, here, however. It is known that awareness accompanies errors but not if awareness precedes (i.e. causes) errors, or results from them, although both may occur. An explanation of the rank order of accuracy obtained with the left hand continuous task could be given in these terms; the use of the non-preferred hand might make the responses unfamiliar, so that the attention would be directed to them rather than to the display alone. Again, in the two-handed continuous task the breaking up of the attention and the conflicting requirements of the two pointers could have had the same effect.

Further evidence which bears on this point is given by Vince (1950). Here, where subjects were given a considerable amount of practice with the "unexpected" discontinuous task and without visual knowledge of results, it was found that learning was accompanied by a decrease in the subject's awareness of his correct responses, and an increase in the speed with which he corrected his errors. That touch typists as they become proficient tend to lose awareness of their responses *except when a mistake is made* has been pointed out by Book (1925).

Awareness of the response, appears, then, to be associated with difficulty or a failure to adapt successfully, perhaps a necessary part of a new skill but one which is useful only in the early stages.

VI

Conclusions

i. In handling any mechanism where the movements of a control knob, lever or handwheel are required to bring about corresponding movements of a controlled part of the machine, there are for the majority of people preferred directional relationships between the control and display. In simple cases "expected" relationships correspond to those encountered in daily life, which are practised by most people throughout their lives. However, there are more ambiguous relationships which correspond less with everyday experience, as when display and control are not in the same plane, and here there is not likely to be unanimity as to which is the "expected" relationship.

ii. These preferred relationships may or may not be reflected in performance. In general, when the subject is not hurried, or is able to concentrate his attention on a task which has but one aspect (as in one-dimensional tracking), the directional relationship is unimportant (or the difficulty inherent in it may be overcome); but a fast stimulus rate or an increase in the complexity of the task, or decrease in its familiarity may cause errors if the directional relationship is "unexpected" or ambiguous (or may render the operator unable to deal with the difficulties inherent in the task).

iii. Under such conditions a task where the directional relationship is "unexpected" will take longer to learn than one where it is "expected."

iv. When performance is affected by a non-preferred relationship, the task becomes one where the cognitive element plays a large part; for this reason there is evidence that such a performance can be predicted on the basis of intelligence; high intelligence correlating with a good performance and a rapid rate of learning and low intelligence with a poor performance and a slow rate of learning.

v. Conditions have been found where it is possible to reverse the relationship between control and display without affecting the subject's performance. This has generally been found to occur only when a rapid series of responses is required, and also only when the subject has not been aware of the reversal.

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THE EFFECT OF KNOWLEDGE OF RESULTS ON LEARNING AND PERFORMANCE

IV. THE DIRECTION OF THE ERROR IN VERY SIMPLE SKILLS

BY

VALERIE DEES and G. C. GRINDLEY

(*From the Psychological Laboratory, Cambridge*)

Re-examination of the data discussed in previous papers of this series shows a greater tendency toward "overshooting" when the time interval between trials, in some of the experiments, is short than when it is longer. The subject tends to make a bigger movement or exert more pressure with short intervals. This seems to be true with or without visual knowledge of results. On the other hand, with the experiments in which the task was to press a key for a given short interval, the effect was not conclusively shown.

A hypothesis is put forward to explain these results in terms of proprioceptive adaptation.

I

INTRODUCTION

IN the earlier papers of this series (Elwell and Grindley, 1938, Macpherson, Dees and Grindley, 1948, and Macpherson, Dees and Grindley, 1949) we have treated the results in a conventional way, plotting and tabulating errors in learning and performance curves without regard to their algebraic sign. Thus "overshooting" or "undershooting" the target by five units would count as equal errors. A change in the conditions may, however, tend to make the subject give a bigger or smaller response than he did in the previous trial, without necessarily altering his average error, e.g. by making him overshoot the target by the same amount as his previous undershoot. The very wide scatter between individual readings may make such an effect pass without notice. With this in mind we have re-examined the data for the effect of varying the time interval between trials.

Three of the experiments so far reported, namely those that have been called "Knob Turning," "Pressure" and "Time," will be discussed here. The other experiments are omitted because there was no adequate control of the time interval between trials.

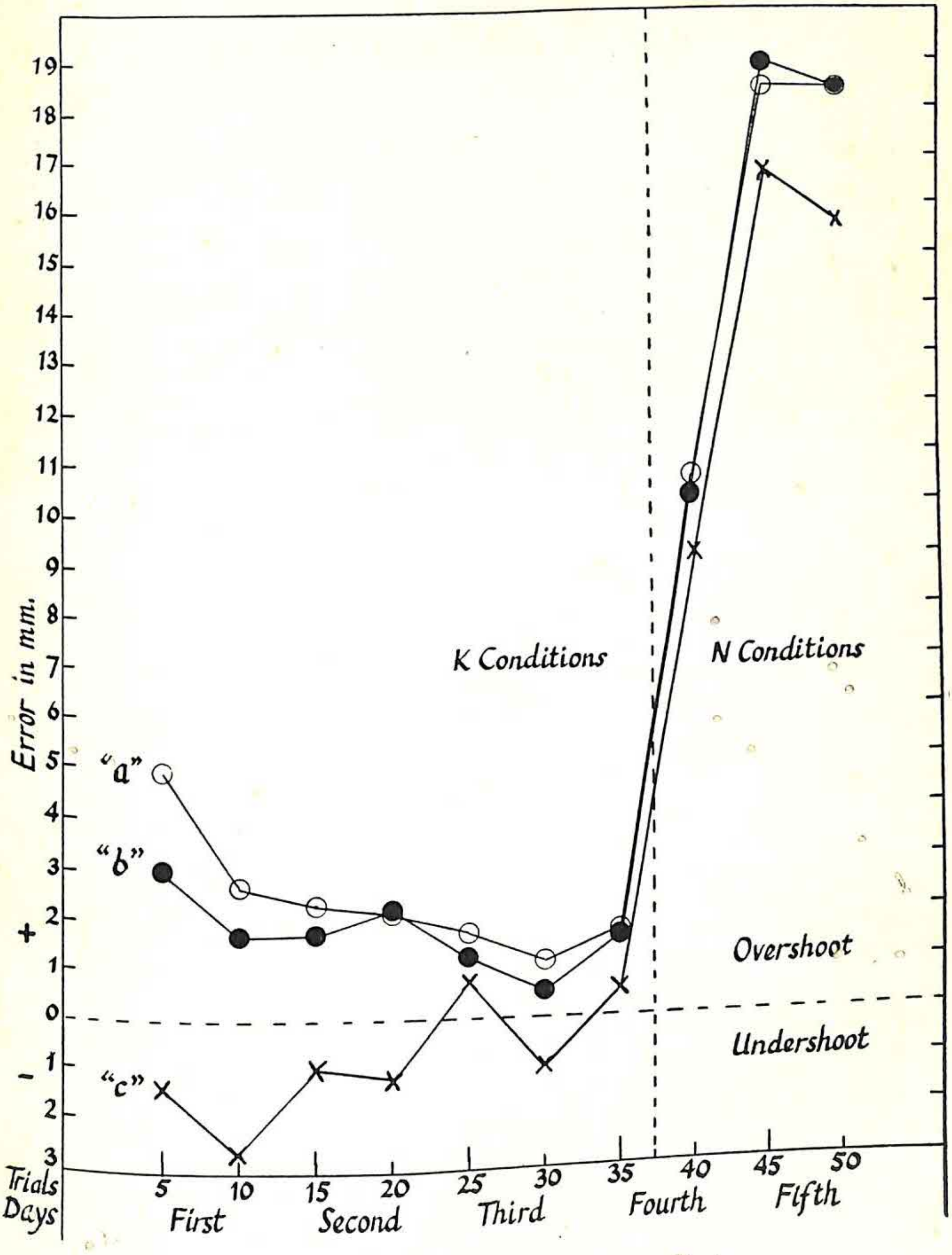
II

RESULTS

A. *Knob Turning.* (For details, see Mapherson, Dees and Grindley, 1949, p. 168.)

In this experiment, in order to hit the bull's eye, the subject's task was to turn a knob through 20° and then immediately return it to zero. He could then, for K ("knowledge of results") conditions be shown the amount and direction of his error by the position of a spot of light on a scale; or for N ("no knowledge of results") conditions, this information could be withheld. A record of his actual movement was made by a pen writer, and it was possible to make the trials as frequent as one every 1·8 seconds. As was shown in the previous paper there was, under K conditions, a very slight tendency to improve more rapidly, in terms of number of trials, with short time intervals than with longer ones. Under N conditions deterioration was more rapid with short time intervals.

FIGURE 1



Each point = Average of 5 trials for 21 Subjects.

○ = Trial after 1·8 seconds interval.

● = " " 7·5 " "

× = " " 33·0 " "

If now, taking the same data as before, we include the algebraic sign of the error in our calculations, the general trend of the results shows a very marked qualitative difference between the longer and shorter intervals. Thus for the 21 subjects discussed in the earlier paper, Figure 1 shows the average error for time intervals of 1·8, 7·5 and 33·0 seconds between consecutive trials, the algebraic sign being included. As will be seen from the legend, each point represents the mean of 105 determinations. Half-way through the fourth day the subjects were changed to N conditions. It will be seen by inspection that both for K and N trials, curve "a" (1·8 second intervals) is always above curve "c" (33·0 second intervals), whilst curve "b" (7·5 second intervals) nearly always occupies an intermediate position. This means that after a short interval the subjects tended to turn the knob farther than after a long interval. It will also be seen that with practice under K conditions the curves tend to approximate to a point near the level of the bull's eye.

When knowledge of results is removed the tendency to overshoot is very marked with all three time intervals, but it is even greater in extent with the short time interval than it is with the longer one. The tendency to turn the knob farther the shorter the time interval still persists.

With the wide scatter of individual readings it is, of course, necessary to have some statistical check on the reliability of the results. We have considered than an enumeration of cases in which one "shot" is greater or less than the previous "shot" is an appropriate way of approaching this question. (This avoids any assumption that the scale used is, in any sense, either physiologically or psychologically, a linear one.)

Table I shows the number of cases under K conditions where an increase or decrease of angle of turn occurred, as compared with the immediately preceding angle of turn, for each of the three time intervals. It must be remembered that all the subjects had time intervals in *cycles* in which the order was 1·8, 7·5, 33·0, 1·8, 7·5, 33·0 seconds, and so on.

TABLE I

Time intervals (seconds)	1·8	7·5	33·0
Cases of increase in extent	465	334	261
Cases of decrease in extent	239	353	429

Table II shows the corresponding figures under N conditions for the same subjects.

TABLE II

Time intervals (seconds)	1·8	7·5	33·0
Cases of increase in extent	272	206	189
Cases of decrease in extent	153	214	241

It will be seen that these tables show a very striking difference between the long and short time intervals. On comparison with the data already published (Macpherson, Dees and Grindley, 1949, pp. 169 and 170) the effect of including the *direction* of the change is quite clear.

Another way of demonstrating the point in question is to compare the data, as shown in Figure 1, with the same data replotted *without* the sign. The latter is done in Figure 2. Anyone inspecting that figure would be unlikely to think that time intervals had much effect under K conditions. For N conditions the results are not very different from those shown in Figure 1 because, in any case, nearly all the trials resulted in "overshoots."

B. *Pressure.* (For details, see Macpherson, Dees and Grindley, 1948, p. 75 and 1949, p. 172.)

In this experiment the subject task, in order to hit the bull's eye, was to press a lever against an increasing force until he attained a pressure of about 2 lbs. weight.

It was found, as has been discussed in the preceding paper of this series, that there was a very marked tendency to improve more rapidly when the time interval was short (10 seconds) than when it was longer (60 seconds). When knowledge of results was removed deterioration was more rapid with the shorter interval.

If we now analyse the results, taking into account the sign of the error, we find (just as in the Knob Turning experiment) that there is a very marked tendency to push harder after the short interval than after the long one. Table III shows the number of cases of increase or decrease (compared with the previous trial) of pressure exerted with each interval under K conditions.

TABLE III

Time intervals (seconds)	10	60
Cases of increase of pressure	135	40
Cases of decrease of pressure..	40	118

Similarly Table IV shows the number of cases under N conditions.

TABLE IV

Time intervals (seconds)	10	60
Cases of increase of pressure	447	282
Cases of decrease of pressure..	293	435

Over 75 per cent. of the trials, under N conditions, were "overshoots" and in many cases the overshooting was so great as to be beyond the range of the scale used. (When two successive shots were both off the scale this pair could not be included in Table IV.)

C. *Time.* (For details, see Macpherson, Dees and Grindley, 1948, p. 72, and 1949, p. 171.)

In this experiment the subject's task was to press a morse key so that, in order to hit the bull's eye, he had to make contact for about 0·7 second. When he had released the key his accuracy could be shown to him on a galvanometer scale. As has been pointed out (Macpherson, Dees and Grindley, 1949, p. 171), intervals of 12 and 60 seconds between trials produced only a slight difference in their effect under both K and N conditions. Re-examination of the data, taking the sign of the errors into account, also fails to show any convincing difference between the effects of these two time intervals. This may possibly be due to the fact that the movement only took about 0·7 second, and the shortest interval between trials, owing to apparatus difficulties, was 12 seconds. It is possible that had there been greater massing of trials different results would have appeared, but on this we have no evidence.

D. *The Effect of 24-Hour Intervals.*

In all three of the experiments discussed above, the subjects were given series of trials on each of four or five successive days. We therefore have some data on the effects of a 24-hour interval under both K and N conditions, although, obviously the number of cases which can be taken into account is much smaller than those for the shorter intervals already considered. For instance, if one subject has 31 trials on each of five days (as was the case in the Knob Turning experiment), we can make a total of 150 comparisons per subject for the time intervals discussed above; but we can only make 4 comparisons per subject for the effect of 24-hour intervals.

Inspection of the data, while confirming the tendency described in previous papers of this series, to become less accurate during a 24-hour period, does not show any consistent trend in the (algebraic) direction of this deterioration. This may be merely owing to lack of sufficient data, but if there was any very strong directional tendency some indication of this might have been expected to show in the results.

III

Discussion and Conclusions

The results of previous experiments, which are reviewed in this paper, show very clearly the importance of taking into account the direction as well as the amount of the error in any curve of learning or performance. For instance, in the "Knob Turning" experiment, if we consider only the amount of the error, as in Figure 2, we should infer that the time interval between trials has very little influence on the results. If, however, we include the algebraic sign of the error, as is done in Figure 1, we see at once that the time interval has a very large effect. This, apart from its application to the present problems, is a general point in the methodology of the study of learning and performance. Taking the analogy of psycho-physical experiments, this is only like saying that the Constant Error as well as the Average Error should be considered.

More interesting, perhaps, are the hypotheses suggested by the results when the direction of the error is taken into account. *One* such hypothesis may be roughly summarized as follows. Let us assume:

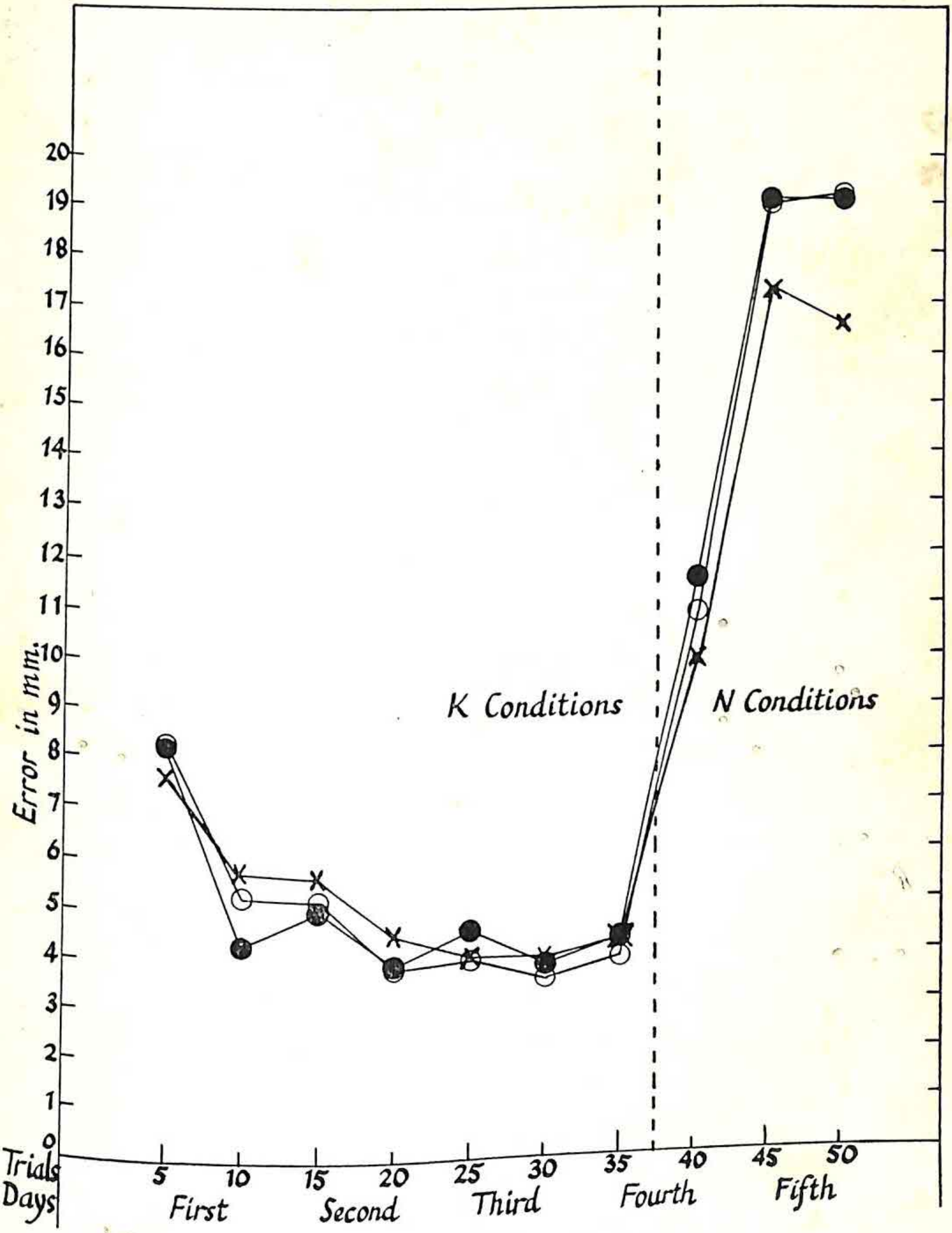
(*a*) That when a subject is trying to repeat a movement (e.g. when he has hit the bull's eye in the previous trial) he is trying to obtain proprioceptive sensations which "match" (cf. Bartlett, 1932, p. 20 et seq.) his "memory trace" of those in the previous trial.

(*b*) That the proprioceptive mechanism is susceptible to adaptation (cf. Adrian, 1928), i.e. during the period immediately following a response the receptors, or some more central part of the mechanism will be less sensitive. The degree of adaptation will, of course, decline with the time since the last response.

(c) That the "memory trace" left by the proprioceptive stimuli (like those of some visual forms studied, e.g. by Tsao, 1949) becomes less precise with lapse of time, but that there is no marked directional trend in such forgetting.

Making these assumptions, none of which seems to be improbable, it at once becomes clear why in the Knob Turning and Pressure experiments a short interval

FIGURE 2



Each point = Average of 5 trials for 21 Subjects.

○ = Trial after 1·8 seconds interval.

● = " " 7·5 " "

× = " " 33·0 " "

between trials tended to produce a greater response in the second trial than in the first one; since, owing to the adaptation of the proprioceptive system, the second response, when it reaches equality with the first, would still feel too small to the subject and he would tend, therefore, to increase it. (Any corrective action towards the direction of undershooting would not have been registered on any of the pieces of apparatus discussed—since each of them recorded only the *maximum* reached.) With longer time intervals (e.g. 30 seconds) the adaptation would have diminished. And with very long intervals (e.g. 24 hours) would have disappeared.

With the Time experiment this adaptation effect would not be expected to any marked degree because of the very short duration of the subject's movement compared with the minimum interval which was possible between trials. It may also be that the short absolute duration of the movement made correction while the action was in progress impossible or very difficult (cf. Hick, 1949, and Vince, 1948).

The hypothesis suggested makes no claim to explain the general mechanism of "learning," or the differences between what have been called K and N trials, or the "incentive" and "directive" effects. A complete theory should obviously link these findings with the many investigations of phenomena such as the "time error," muscular "after contraction," and "positive" and "negative" after images. But since these investigations do not yet form a coherent whole, we have not attempted to discuss them here; and have confined ourselves to a simple hypothesis about our present data. What this suggests is a possible explanation of the observed effects of time intervals between trials; it is, of course, incomplete and unproved.

Several other lines of approach to the problem would be possible, but the justification for this one is that it seems to fit with a number of observed facts. Also, since the suggested locus for the underlying process is in the proprioceptive mechanism, it makes possible some predictions about what ought to happen in more complex skills, where more than one muscle group, or more than one limb, is involved.

We wish to express our thanks to Professor Sir Frederic Bartlett for many suggestions, and to the Rockefeller Foundation for financial aid.

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A PRELIMINARY REPORT
ON A NEW AUDITORY AFTER-EFFECT

BY

J. A. DEUTSCH

(From the Institute of Experimental Psychology, Oxford)

It is found that protracted monotic stimulation at a certain frequency lowers the DL of that frequency for the contralateral ear by some 47 per cent. There were eight subjects and six controls; the frequency was 1,000 c.p.s. at 40 d.b.s. It is suggested that this after-effect is analogous to figural after-effects in vision. (Some further related findings, as yet preliminary, are also reported).

I

INTRODUCTION

KÖHLER and Wallach show that changes occur in the visual field after prolonged uniform stimulation and that these are central (1944). If a figure is fixated for a prolonged period, other figures falling on or near the same part of the visual field immediately afterwards recede from where the contours of the first figure were first located. It is therefore of theoretical interest to see whether analogous changes occur in other sense modalities and more especially in hearing.

II

METHOD

(1) *General considerations:* The actual methods of determining this was to measure the DL monotically at a certain frequency (1,000 c.p.s.) and then to stimulate the other ear at the same frequency for a prolonged period. Though not providing a direct test the method is simple and uses a minimum of apparatus. It seeks to verify the prediction based on the visual analogy in the following way.

A contour in visual space tends to push away other contours and make the interval between them seem larger. Therefore a tone in the auditory scale ought to push away the neighbouring tones and increase the tonal interval between them. Now it has been found that the ratio of the physical magnitude of equal-appearing intervals is roughly equal to the ratio of the physical magnitude of the DLs found respectively in the two intervals (Stevens and Davis, 1938). If then the phenomenal tonal distance in any region of the frequency scale does increase, the size of the DLs there ought to decrease. The DL at a certain frequency should diminish after prolonged stimulation at that frequency.

(2) *The procedure of investigation:* The procedure had two aims:

(*a*) to secure a clearly central effect;

(*b*) to avoid too much stimulation when testing for the DL initially, for fear of introducing the predicted after-effect prematurely.

In order to achieve the first aim, a tone of 1,000 c.p.s. was sounded in one ear through one of a pair of earphones, 40 d.b.s. above threshold. This intensity was used in both the DL determinations and also in the prolonged contralateral stimulation. A tone of this intensity is inaudible through the other ear under these conditions. The tone was sounded by pressing a switch.

In order to achieve the second, the method of minimal changes was used in preference to the method of constant differences, as it is more economical. Eight series, four ascending and four descending, were generally presented to each subject in order to determine the initial DL. The two notes were always played in the same order, i.e. the higher note was always sounded after the note of 1,000 c.p.s. The subject was instructed to report either "same" or "different" in pitch. These further points in procedure were adopted because they proved to be the least confusing to naïve subjects.

The subject was seated facing away from the experimenter and the control dial. Each tone was presented for approximately one second. The tone to be compared was presented about half a second later for the same length of time. These times are approximate because the switching was done by hand.

After the DL had been thus determined, the tone of 1,000 c.p.s. was sounded in the contralateral ear (monotically) also at 40 d.b.s. above threshold for four minutes.

At the end of this period, the DL at this frequency was again determined for the original ear, using exactly the same procedure as in the first instance. With some subjects more than eight series were presented.

The control group was subjected to the same procedure, except that a note of 600 c.p.s. 40 d.b.s. above threshold was sounded instead of the one at 1,000 c.p.s. The experiment was not carried out in a sound-proof room.

(3) *Subjects:* The experimental group consisted of eight subjects roughly between the ages of 18 and 24. The control group consisted of six subjects of the same age group. Both groups were naïve, i.e. they did not know what results to expect. Some subjects who were hopelessly inconsistent in making judgments were rejected both as subjects and controls. A subject was deemed to be inconsistent when the distribution of "same" judgments was non-modal.

III

RESULTS

After the DLs both before and after contralateral monotic stimulation had been obtained, it was determined by Student's *t* test, using a formula for small numbers, whether the DL for each subject was significantly lower in the second instance than in the first. The 0·05 level of confidence was adopted as significant.

TABLE I

RESULTS OF EXPERIMENTAL GROUP

Subject	*DL before stimulation (in c.p.s.)*	*DL after stimulation at 1,000*	*Decrease in c.p.s.*	*% Decrease*	*t, with degrees of freedom*	*Whether significant*
1	1009·5	1005·65	3·85	44·74	4·9 22 df	yes
2	1007	1003·88	3·12	44·57	2·63 22	yes
3	1006·75	1003·91	2·84	42·33	6·89 17	yes
4	1019·7	1010	9·7	49·24	4·93 16	yes
5	1012·5	1006·5	6	48·00	4·03 14	yes
6	1013·25	1005·8	7·45	56·33	2·16 14	yes
7	1011	1006·7	4·3	39·10	2·40 13	yes
8	1002·37	1001·32	1·05	44·43	2·08 18	no
Average	1010·26	1005·44	4·82	46·98		

The results for the experimental group are set out in Table I. Of the eight subjects in this group, seven showed a significant decrease. The one subject who did not show a significant decrease, nevertheless showed a decrease.

The results of the control group are given in Table II. The six subjects in this group varied randomly, some in the direction of a diminution of threshold, others fluctuating upward.

TABLE II

RESULTS OF CONTROL GROUP

Subject	*DL before stimulation (in c.p.s.)*	*DL after stimulation at 600*	*Decrease in c.p.s.*	*% Decrease*	*t, with degrees of freedom*	*Whether significant*
1	1008·75	1007·5	1·25	14·28	0·88 14	no
2	1005·75	1007·25	−1·5	−26·08	1·44 14	no
3	1009·25	1008·25	1	10·08	0·78 14	no
4	1013·5	1014	−0·5	−3·70	0·35 14	no
5	1008	1007·5	0·5	6·25	0·41 14	no
6	1002·25	1002	0·25	11·11	0·14 14	no
Average	1007·92	1007·75	0·17	2·14		

It is clear from the results that there is marked decrease in DL in the experimental group.

The control group had an initially lower DL than the experimental group. But it is found that this variation between the groups could quite easily occur by chance ($t = 0{\cdot}035$ with 12 degrees of freedom, even when we treat each mean as a measurement and ignore the scatter). Therefore the control is a true one.

IV

Further investigations of the effect (which are as yet preliminary):

These indicate that:

(*a*) The DL can be lowered in this manner at any heard frequency.

(*b*) Both the ascending and the descending DLs decrease by the same amount.

(*c*) A whole small range surrounding the prolonged stimulus tone becomes more sensitive to alteration of pitch.

(*d*) Subjects whose DL for a particular frequency has been lowered by the described procedure, exhibit a rapid return to the lower DL in the course of re-testing 24 hours later. This occurs without any contralateral stimulation and merely as a result of the stimulation inherent in the DL determination itself.

(*e*) If a subject is asked to bisect a tonal interval before and after contralateral monotic stimulation on the base note of the interval, the estimated midpoint shifts towards the base note.

V

DISCUSSION

The contention may be held that the results obtained are due to the practice effect, an already well-known factor in DL measurement. This is clearly invalid. Both the experimental and control groups had the same amount of practice in the initial determination. If one may speculate on the basis of these results, may not the so-called practice effect be in reality due to prolonged stimulation and not to any practice or learning?

Wyatt (1945) claims that pitch-discrimination can be improved with training at particular frequencies, with some transfer to the octaves. The present findings suggest an alternative causal factor, and one which, if taken into consideration, would explain the divergence of experimental results on the influence of training in discrimination. If sharp improvement occurs as a result of prolonged stimulation "training" may or may not have an effect depending exactly on the amount, duration, and spacing of the "practice." This might give rise to "contradictory" results.

The after-effects discovered in this experiment would indicate that there is something analogous to visual figural after-effects in hearing.

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BOOK REVIEWS

Physiological Mechanisms in Animal Behaviour. [Symposia of the Society for Experimental Biology, No. IV.] Cambridge University Press, 1950. Pp. vii + 482. 35s. net.

It is an old saying that there are no *real* frontiers between allied sciences. If we make use (as we all do in our teaching or research about the living organism) of a large and—perhaps deplorably—increasing number of titles for different branches of study, this can only be justified on grounds of practical or administrative convenience. The present book illustrates the point well in the case of the relationship of "psychology" to "physiology" or "zoology"; for many of the authors would claim to speak only as specialists in particular branches of one or the other of these sciences. Yet the result, if not a completely coherent whole, serves to emphasize the similarities rather than the differences between the methods and results of people who work under quite different academic labels.

The book gives the text of the papers read at an international symposium (organized jointly by the Society for Experimental Biology and the Association for the Study of Animal Behaviour), which was held in 1949. As there were 21 contributors, discussing different things, coming from different countries, and each giving his own views on his own subject, space does not permit anything like the detailed commentary which most of these articles deserve. The book, however, is divided into four sections, about each of which a word may be said.

The first, on "The Range and Capabilities of the Sense Organs," contains very valuable summaries—by authoritative writers—of present views on hearing, vision, proprioception, and the functions of the labyrinth. While admiring these one may regret that time did not permit similar contributions on the tactile, thermal or chemical senses.

In the second section, on "Central and Peripheral Control of Behaviour Patterns," we have the exciting mixture of papers by E. D. Adrian, Paul Weiss, James Gray, G. P. Wells and E. von Holst. There is nothing which could be called an agreed conclusion, but there is much in each which needs a lot of thought, e.g. the usually super-cautious Adrian feels driven to say ". . . in the cerebral cortex there are no through routes. . ."

The third section, on "Instincts, Taxes, etc.," is a rather mixed bag. It contains eight papers which vary from the very specific (e.g. P. H. T. Hartley on recognition in birds—a paper which has immense implications for the theory of perception) to the very general views of K. Z. Lorentz on "Comparative Method" or of N. Tinbergen on "Hierarchical Organization." C. F. A. Pantin's account of the lower invertebrates, or E. A. Armstrong's essay on "Displacement Activities" (in birds) would each need separate reviews.

The last section, on "Learning," contains only four papers, but all of these are likely to be interesting to most psychologists. W. H. Thorpe surveys the whole problem from what might be called a "Lloyd Mogan + Thorndike + Lorentz" point of view, while J. Konorski does much the same from a Pavlovian angle. Then come R. B. Boycott and J. Z. Young with the octopus, and the intriguing effects of lesions in its nervous system. Finally we have K. S. Lashley "In Search of the Engram," and we must admit that in its power to demolish current theories the old scalpel has not lost its sharpness. One remark will suffice as an illustration: "I sometimes feel, in reviewing the evidence on the localization of the memory trace, that the necessary conclusion is that learning just is not possible. . . ." Nevertheless, Lashley goes on to say ". . . learning does sometimes occur." Perhaps one is wrong in being reminded of Galileo or Einstein.

Anyone who wants to know what biological experts think about behaviour and the causes of its many varieties ought to have this book. But it is not a text-book; it is a collection of views. Those who attended the symposium probably have happy memories of the lack of distinction between academic "subjects," as well as of the high standard of discussion. Thanks are due to J. F. Danielli and R. Brown for their admirable work as editors.

It is worth adding that during the period of this symposium a series of "round-table conferences" was held to discuss nomenclature in animal behaviour, in an attempt to find whether a more uniform terminology could be agreed. The terms discussed included many which are commonly used in general psychology (e.g. "instinct," "inhibition," "conditioning," "learning" and "insight"). A concise summary of these discussions has been prepared by W. H. Thorpe, and will shortly appear in the *Bulletin of Animal Behaviour.*

G. C. G.

Handbook of Psychological Research on the Rat. By N. L. Munn. London: George G. Harrap & Co., Ltd., 1950. Pp. xxvi + 598. 35s. net.

This is a most valuable and comprehensive summary of work on all aspects of rat behavior. It is well arranged, and has an admirable bibliography of about 2,500 papers. The title is much more appropriate than that of the original "An Introduction to Animal Psychology" (1933)—of which this is a much expanded and much more up-to-date version. The book is about rats only; but about them it is encyclopædic.

Proceedings of the Experimental Psychology Group 1949-50

10th December, 1949.—3rd Annual General Meeting in London. "The Comparative study of Psychological and Physiological Effects of Electroshock Convulsions," by R. W. Russell. "'Equivalent Rooms' with some Remarks on the Transformations involved in Visual Perception," by R. C. Oldfield.

20th May, 1950 (London).—"The Subjective Judgment of the properties of Deformable Materials," by R. Harper. "The Testing of Abstraction, with particular reference to Deterioration in Schizophrenia," by K. R. L. Hall. *2nd Session:* "The Experimental Study of Rumour," by T. M. Higham (by invitation).

21st-22nd July, 1950.—Extended Meeting at Cambridge. *1st Session:* "Experimental Finger Dyspraxia," by C. V. Jackson (by invitation) and O. L. Zangwill. *2nd Session:* "Programme for Experiments on Thinking," by Sir Frederic Bartlett (by invitation). *3rd Session:* "Perceptual Anticipation in Harmonic Tracking," by C. Poulton. "A Probability Approach to Trial and Error Learning," by A. R. Jonckheere (by invitation). *4th Session:* "Investigation into changes in Autonomic Responses following Pre-Frontal Leucotomy," by A. Elithorne and M. F. Piercy (both by invitation).

29th-30th September, 1950.—Extended Meeting at Oxford. *1st Session:* "Derived Activities," by N. Tinbergen (by invitation). *2nd Session:* "Speed Stress," by R. Conrad (by invitation). "Variation in Reflex Blink Rate during Visual-Motor Activities," by G. C. Drew. *3rd Session:* Short reports on work in progress at the Oxford University Institute of Experimental Psychology by J. A. Deutsch, R. F. Benton, K. Danziger and A. J. Weir (all by invitation).

16th December, 1950.—4th Annual General Meeting in London. "The EEG in the Study of Personality Disorders," by D. A. Pond (by invitation). "Some material for a Discussion of the Constancy of Size," by R. B. Joynson.

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SENSORY PHENOMENA IN EXPERIMENTAL NERVE BLOCK

BY

D. C. SINCLAIR AND J. R. HINSHAW*

(From the Department of Human Anatomy, Oxford University)

In the course of a series of blocks of the ulnar and lateral popliteal nerves of volunteers a study was made of the manner in which the responses of the subject to sensory stimulation become progressively altered and finally obliterated. These changes are described, together with the manner of recovery, and an account is given of various sensations induced by the blocking and the recovery processes. The relevant literature is briefly reviewed and possible factors involved in these phenomena are discussed.

I

Introduction

It has long been known that the normal sensibility of an area of skin can be progressively reduced and eventually abolished by the application of various physical or chemical agents to the nerve trunk supplying that area. This knowledge remains largely empirical, and there is little information as to the exact mode of action of these anæsthetic agents. Nevertheless, the production of regional or local anæsthesia has been used as an experimental tool in the study of the mechanisms of cutaneous sensation.

Under these circumstances, the different types of sensory experience tend to disappear serially rather than simultaneously, and the study of sensory dissociation of this kind contributed materially to the acceptance of four primary modalities of sensation by von Frey and his school. Indeed, the main object of the earlier workers was to demonstrate that the disappearance of a given "modality" of sensation was a process independent of the disappearance of the others. Subsequent investigators using the method have tended to concentrate their attention on particular details, such as the relation of the disappearance of two-point discrimination to the time of disappearance of touch sensibility, or the relationship of itching to pain sensibility. The result has been that no full account has appeared of many phenomena "incidental" to such experiments. These phenomena often appear to be inherent in the process of nerve block, however it may be initiated, and a study of them may well throw light on a number of problems connected with cutaneous sensation. It is the intention of this paper not to deal exhaustively with any one of them, but rather to provide as complete a picture as is possible at present of the various sensory findings in a series of nerve blocks induced by procaine, cold and compression of the limb.

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* Rhodes' Scholar, Oklahoma and Hertford College, Oxford: now at Strong Memorial Hospital, Rochester, N.Y.

II

MATERIAL AND METHODS

The 31 subjects used were either medical students or medical research workers; only four had any previous experience in the assessment of cutaneous sensation. The skin areas investigated were those supplied by the ulnar nerve and the lateral popliteal nerve.

Technique of inducing blocks.

For the procaine blocks a solution of procaine hydrochloride in strengths varying from 1½ to 2½ per cent. was injected round, but not into, the ulnar nerve as it passes behind the

TABLE I

NUMBER OF BLOCKS

Subject	*Ulnar blocks*			*Lateral popliteal blocks*		
	Procaine	*Cold*	*Compression*	*Procaine*	*Cold*	*Compression*
C. A. W.	1					
J. H. F.	1					
H. B.	1					
I. A. Y.	1					
R. H. P.	1					
H. M. H.	1					
J. P. R.	1					
M. E.	1					
D. H. C.	1					
K. M. O.	1					
R. B. E.	1					
W. F. B.	2	1				
L. W.	2	1				
C. W.	2	1	1	1		1
R. I.	2	1	1			
D. S.	1	1	93		1	4
W. C.	1	1	1			
J. D. W.	1	1	1			
R. W. B.	1	1	1			
D. R. W.	1	1	1			
J. R. H.		1	1		1	
D. G. C.			1			
R. J.			1			
C. B.			1			
W. H. F.			17			
G. W.			21			
A. W. D.			15			
R. B. I.				1		
E. J. B.				1		1
G. A. R.				1		1
R. B. E.				1		1
TOTAL	24	10	156	5	2	8

elbow, or the lateral popliteal nerve as it passes over the neck of the fibula (Sinclair & Hinshaw, 1950).

The compression blocks were induced by inflating a sphygmomanometer cuff 12 cms. wide round the arm just above the medial epicondyle of the humerus, or just below the knee-joint (Sinclair & Hinshaw, 1951a). The pressure used was 200 mm. Hg. in both cases; for the arm compressions the limb hung vertically at the side (Sinclair, 1948), while for the leg compressions the subject lay on his back with the leg horizontal.

Cold blocks of the ulnar nerve were induced by immersion of the elbow in a trough containing chopped ice and brine at a temperature of from 0° C. to minus 2° C. For the cold blocks of the lateral popliteal nerve, brine from a reservoir containing chopped ice ran slowly through a copper tube strapped over the neck of the fibula, the temperature in the tube being from 2° C. to 4° C. (Sinclair & Hinshaw, 1951b).

Table 1 shows the number of times each subject submitted to a given type of block. Most of the compression blocks were not maintained for longer than 20 minutes, and only 14 of the total of 164 were carried sufficiently far to enable a determination of the order of loss of sensory modalities to be made. On the other hand, the 12 cold blocks and the 29 procaine blocks were all taken to this point.

Technique of sensory examination.

Within the autonomous zone of the ulnar nerve, six circles 1 cm. in diameter were stamped out with marking-ink (Figure 1); for the most part sensory examination was confined to the three marked 1, 4 and 6 in the diagram. A similar set of three circles was used for the lateral popliteal nerve (Sinclair & Hinshaw, 1950), and in this case the hairs in the region of the testing areas were clipped short (not shaved). This was done in view of the observed difference in behaviour between skin touch and hair touch in compression blocks (Sinclair, 1948). To these circles stimuli were applied at intervals, the order in which the circles were tested by any given stimulus being randomized. The reactions to four different stimuli were investigated. The first of these was a piece of nylon suture

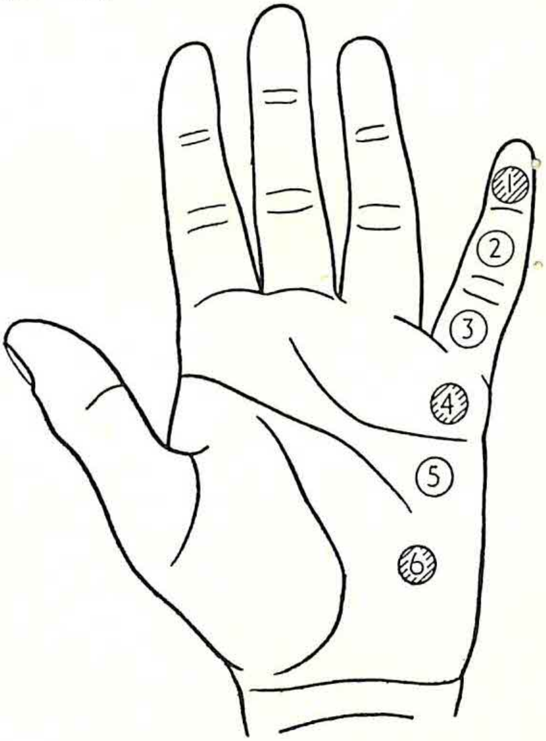

FIGURE 1

Sensory testing areas in the distribution of the ulnar nerve. Each area is 1 cm. in diameter and is stamped out on the skin with marking ink.

material (Figure 2) delivering a punctate stimulus within the testing circle, care being taken to avoid skidding or slipping of the end of the nylon thread on the skin. Such a stimulus could invariably be appreciated in the ulnar testing areas, but the percentage response in the lateral popliteal areas varied considerably from subject to subject, and, if

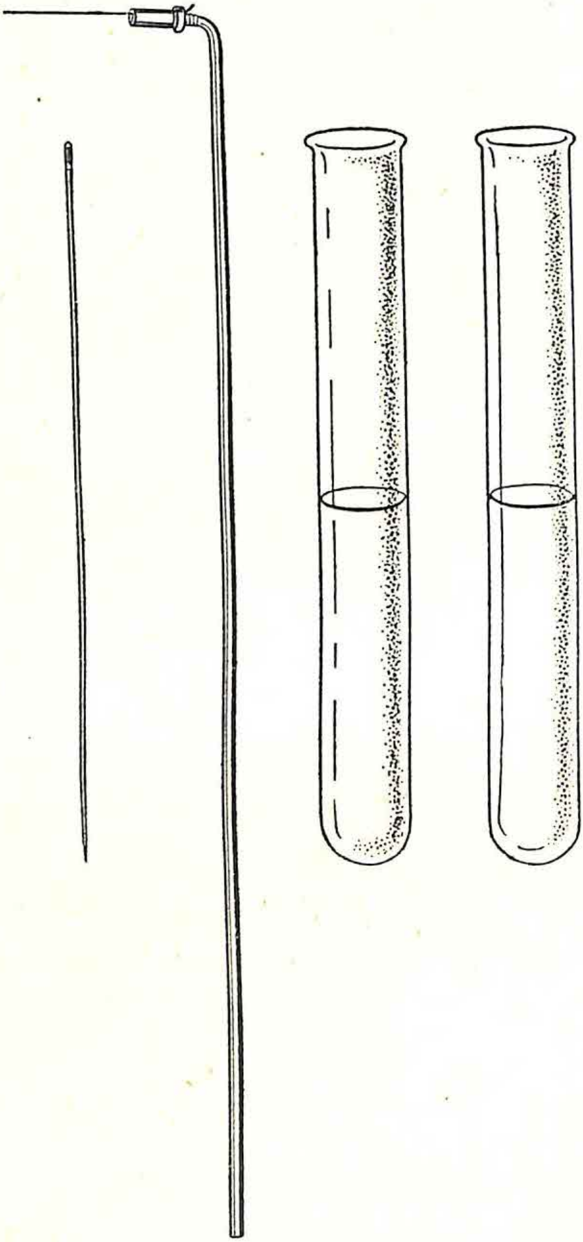

FIGURE 2

Standard Sensory Stimuli: The round-bodied needle is 12·5 cms. long. The piece of No. 2 nylon suture material is mounted on a holder 25 cms. long made from a bicycle-spoke; the free portion of the suture is 2·5 cms. in length, 0·17 mm. in diameter, and is capable of holding down a weight of 500 mg. The test-tubes are 1·5 cms. in external diameter and 12·5 cms. long, and are half-filled with water at 45° C. ± 2° C. and 15°C. ± 2° C. respectively. The surface of the tubes in contact with the skin is about 50 sq. mm.

the hairs were avoided, there was frequently no response at all; in such cases a heavier nylon thread was substituted. Alterations in the response to this stimulus were taken to reflect events taking place within the modality of touch.

Events within the modality of pain were investigated by the reaction of the subject to a vertical prick, just not drawing blood, from a round needle (Figure 2). The pricks were delivered at random within the boundary of each circle. This form of stimulus necessarily affects modalities other than that of pain; nevertheless it was taken as the "pain stimulus." In the normal subject the pricks were consistently stated to produce pain when applied to the ulnar territory: in the lateral popliteal area occasional pricks were identified merely as "sharp" or "prick without pain," and it might be necessary to draw blood before the subject would admit to a sensation of "pain."

The testing of temperature sensibility likewise involved modalities other than that of temperature. A glass test-tube half full of warm or cold water and containing a thermometer was allowed to rest by its own weight on the circle to be tested. The temperatures of the water were 15° C. ± 2° C. ("cold stimulus") and 45° C. ± 2° C. ("warm stimulus"). No measurement was taken of the temperature of the outside of the tube, but neither stimulus ever caused any reaction interpreted by the intact subject as pain. Normally, these two stimuli were interpreted as "cold" and "warm" over the greater part of the ulnar territory. Over the palmar surface of the distal phalanx (area 1), however, both were occasionally called "indifferent," and in some subjects it was difficult or impossible to distinguish between the two tubes in this area. In certain instances the findings were similar in the testing areas of the lateral popliteal territory.

Every subject was told that the object of the experiment was to investigate the way in which sensation disappears from an area of skin when its nerve supply is blocked, and warned that if he experienced any "spontaneous" sensations in the area blocked during the experiment he was to report and describe them as soon as they arose. He was told no details of what he might expect. During the testing periods he was told which modality was being tested—i.e. "touch," "pain" or "temperature," but was not permitted to watch the application of the stimuli. Routine precautions were taken to guard against the occurrence of "imaginary" responses (e.g. by pretending to test one modality and actually testing another, by announcing but not fulfilling a test, etc.). In the case of the temperature tubes in particular, every effort was made to conceal from the subject which tube was being used. The subject in every instance initially reported his recognition of the fact that a stimulus had been applied, and secondly described his experience in his own words, an abbreviated version of which was taken down in shorthand wherever possible. In the early stages these descriptions were often extremely clear and detailed, but at the height of the block they usually became reduced to the pathetic phrase, "I think there might be something going on somewhere"—a description which indeed it is impossible to better.

In addition to reporting the results of the standard stimulations, the subjects were often asked to describe the result of much grosser stimuli, such as stroking the whole hand, immersing it in hot or cold water, etc. They were also asked to compare the intensity and quality of the sensation caused by a stimulus applied to the blocked area with the intensity and quality of the sensation caused by the same stimulus applied to the mirror point on the opposite limb.

III

OBSERVATIONS

The general course of events in a typical compression block of the arm is set out in Table II, which is derived from a study of the notes on each individual experiment. The time intervals in this table are only approximate, but are given as a rough guide to the usual sequence. In Table III the phenomena noted are classified in terms of their relative frequency; not all of them appeared in every block, but many recurred with such regularity as to make them characteristic of the blocking process. Table III also shows the relative frequency of these changes in the procaine and cold blocks, and it will be noticed that every sensory manifestation occurring in either the procaine or the cold experiments had its counterpart in the compression blocks. On the other hand, the reference of a sensation to a distant point and the occurrence of positional hallucinations, which both occasionally took place in compression blocks, were not noted in either of the other two types of block.

TABLE II

SEQUENCE OF EVENTS IN AVERAGE COMPRESSION BLOCK

(Cuff on Arm above Elbow: Testing in Distribution of Ulnar Nerve)

Time		*Phenomena encountered*
0–5 mins.	(1)	Discomfort under cuff.
	(2)	Tingling referred to hand begins, usually after 1–2 minutes.
	(3)	Sensory testing difficult during tingling, but stimuli may be felt with greater intensity than in control hand.
5–10 mins.	(1)	Discomfort under cuff may pass off.
	(2)	Tingling subsides.
	(3)	Occasional spontaneous sensations of warmth and cold in the hand; occasional pain in the wrist.
	(4)	Little change in subjective response to stimuli.
10–15 mins.	(1)	Spontaneous brief sensations of temperature or pain may recur.
	(2)	Little apparent change in responses to relatively punctate stimuli. Gross stimuli (stroking with hand, etc.) disclose alteration in quality of touch. Hand "feels different," "velvety numbness" begins.
	(3)	Stimuli occasionally "referred" to distant area on same limb.
	(4)	Hand may feel swollen.
15–20 mins.	(1)	Position sense becomes impaired.
	(2)	Subjective responses to touch and pain stimuli begin to alter in quality and intensity.
	(3)	Paradoxical temperature sensations appear.
	(4)	Gradually increasing delay in perception of all stimuli.
	(5)	Gradual prolongation of subjective response to touch and pain stimuli.
	(6)	Occasional "reference" of sensation, usually proximally.
20–25 mins.	(1)	Touch and position sense fail.
	(2)	Cold and warm sensibility may fail.
	(3)	Identification of temperature stimuli affected by repeated testing.
	(4)	Delay, prolongation and altered quality clearly apparent in all sensory reponses which can still be elicited.
	(5)	Paradoxical temperature sensations.
25–30 mins.	(1)	Cold and warm usually fail.
	(2)	Response to painful stimulus grossly altered in quality and duration: occasional failure to respond.
	(3)	Complete motor paralysis.
30–35 mins.	(1)	Warm sensibility may temporarily recover; grossly delayed and altered in quality.
	(2)	Pain may fail.
	(3)	The position of the limb in space may be felt subjectively to have altered, e.g. it may be difficult to convince the subject that his elbow has not spontaneously become flexed.
35–40 mins.	(1)	Warm stimuli frequently identified correctly.
	(2)	Pain usually fails.
RELEASE:	(1)	Often followed by evanescent spontaneous sensations of warm and cold.
	(2)	Pricking paraesthesia in limb is invariable and lasts for varying period.
	(3)	Sensory testing extremely difficult during paræsthesia: mode of recovery thus difficult to assess.

TABLE III

SENSORY PHENOMENA OCCURRING DURING NERVE BLOCKS

PHENOMENON	Type of block			
	Compression	*Procaine*	*Cold*	
1. "Spontaneous" sensations referred to distribution of nerve.	xxx	xx	x	"Tingling" characteristic of compression and procaine blocks. Occasional pain in all three types of block.
2. Increase in intensity of sensation.	x	x	x	Usually temperature stimuli (control not always tested).
3. Delay in appreciation of stimulus.	xxx	xx	xx	Most marked with temperature stimuli.
4. Prolongation of subjective response.	xxx	xx	x	Most marked with painful stimuli.
5. Alteration in subjective quality of touch and pain.	xxx	xx	xx	Most marked with painful stimuli.
6. Paradoxical temperature sensations.	xxx	xxx	xxx	
7. Alteration in response on repetition of stimulus.	xx	xx	xx	Temperature stimuli only.
8. Reference of sensation.	x			
9. Spontaneous temporary recovery.	x		xx	In compression blocks only involves temperature: in cold blocks all modalities involved. Not always tested in compression.
10. Sensory hallucinations regarding position of limb.	x			Usually related to elbow joint.
11. "Spontaneous" subjective sensation referred to distribution of nerve during recovery.	xxx		x	

x = Occasional.
xx = Frequent.
xxx = Characteristic.

Sensations induced by the blocking process.

The most characteristic of such sensations is a tactile paræsthesia occurring soon after the beginning of the block and felt in the territory of the blocked nerve. The occurrence of "tingling" or "formication" during asphyxial blocks of a limb or of a nerve has been noted by several observers (Bastien & Vulpian, 1855; Weir Mitchell, 1872; Dessoir, 1892; Bourguignon & Laugier, 1923; Bazett & McGlone, 1931; Hyndman & Wolkin, 1941; Kugelberg, 1944, 1946a, 1946b, 1948a, 1948b; Merrington & Nathan, 1949), and in 1947 Weddell & Sinclair investigated the nature of this sensation. It is curious that Reid (1931) and Lewis, Pickering & Rothschild (1931), who worked extensively with compression block of the upper limb, should not mention the occurrence of tingling: our own observations suggest that it is almost invariable when the compression cuff encircles the arm. When, however, the cuff is sited on the forearm, it is less effective (Weddell & Sinclair, 1947; Sinclair, 1948; Magladery, McDougal & Stoll, 1950) and tingling does not as a rule appear. Further, in order to elicit tingling of this type by direct pressure applied to a nerve trunk, it is necessary to exert the pressure over a certain minimal length of nerve (Weddell & Sinclair, 1947).

The sensation has a distinctive quality, and the various descriptions offered are very similar. Thus, Kugelberg (1946) calls it "a soft vibrating sensation"; Weddell & Sinclair (1947) refer to "a faint comfortable soda-water sensation," and Merrington & Nathan (1949) describe it as "a soft diffuse tingling." These descriptions closely resemble those of the sensations produced by weak electrical stimulation of the skin or of a sensory nerve trunk (Ebbecke, 1922; Crook, 1932; Thompson, 1934, and others). Indeed it appears that in compression blocks the sensation has its origin in the nerve trunks at the point where they lie under the cuff, and that the operative mechanism for firing them off is one of asphyxia (Weddell & Sinclair, 1947). It is perhaps relevant that Thompson (1934) has shown that tingling induced by electrical stimulation does not depend on the retrograde stimulation of end-organs, and that Lehmann (1937), produced spontaneous discharges in mammalian nerve by asphyxiation with nitrogen after a time lag in reasonable agreement with that of the tingling in cuff experiments.

Introspectively, the sensation appears to be a distortion of touch sensibility, with little suggestion of a painful component, and this view is supported by the experiments of Kugelberg (1946b), and those of Weddell & Sinclair (1947), who found that in four subjects with partial or complete loss of pain sensibility in one upper limb there was little difference in the sensation of tingling produced by cuff compression on the two arms.

Since then, thanks to the kindness of Mr. H. J. Seddon, Mr. G. F. Rowbotham and Dr. Ritchie Russell, one of us (D.S.) has been able to examine 28 patients with lesions affecting the touch or pain pathways on one side of the body. Among these patients was one in whom neurological evidence suggested that the posterior columns of the spinal cord had been damaged; two in whom antero-lateral cordotomy had been carried out; three in which wounds involved the thalamic region; one with thrombosis of the posterior inferior cerebellar artery, and 21 in which wounds involved various areas of the cerebral cortex, resulting in various degrees of disturbance of sensory appreciation. The results suggest that the impulses producing the sensation of tingling are mainly carried in channels other than those subserving pain sensibility and probably travel with touch impulses. Hyndman & Wolkin (1941) noted the occurrence of tingling sensations in response to the inflation of a compression cuff on the leg of a patient in whom the somatic nerves to the lower extremities had been interrupted, and suggested that this tingling might be carried in sympathetic sensory fibres; tingling of this type can, however, occur in a sympathectomized limb (Weddell & Sinclair, 1947).

In the cortical lesions, if there was complete anæsthesia, tingling could not be elicited by a compression cuff; if there was a degree of hypoæsthesia, perhaps amounting only to faulty perception of figure-writing or a reduction in two-point discrimination, tingling was also diminished, or more commonly absent. If there was no sign of disturbance of touch sensibility, in general the tingling was "normal," though in two patients in whom ordinary testing indicated normal touch sensibility tingling was reduced or absent. The areas of cortex concerned with the impulses of this tingling may therefore be the same as those concerned with the sensation of touch, and cerebral defects in the appreciation of touch are possibly always accompanied by a defect in appreciation of the asphyxial tingling. It may even be that a diminution in the sensation of tingling affords a more sensitive test for such defects than the standard testing methods.

While the occurrence of tingling paræsthesia during asphyxial blocking of a nerve has previously been the subject of some comment, there is very little literature on the tingling which frequently occurs in procaine nerve blocks shortly after the introduction of the anæsthetic. This tingling appears, from the comments of those subjects who underwent both types of block, to be qualitatively indistinguishable from the asphyxial tingling, but is of much shorter duration, as would be expected.

Waller (1861) describes as a well-known fact the occurrence of pain in the ulnar area in the initial stages of blocks induced by immersing the elbow in ice-water. Boycott (1902), who immersed his elbow in a freezing mixture, reports "very marked sensory phenomena . . . further prosecution of the experiment became highly undesirable." From this it may be deduced that the phenomena were of a painful nature, but no further description is given. Wolf & Hardy (1941) report the occurrence of "pins and needles" on immersing the whole hand in water below 12° C.; the time relationships of this sensation are similar to those of the sensation induced by an asphyxial block, but the sensation itself appears to be qualitatively different, since it was unpleasant, and on one occasion at least led to withdrawal of the hand; they conclude from their experiments that this sensation is carried in the C fibres. On the other hand, Granit & Lundberg (1947) have shown that in cats cooling of thin strands of nerve-root filaments leads to "a selective large fibre response" more evident in sensory fibres. In the present series of cold blocks we have not observed any tingling or touch paræsthesia during the development of the block, but occasionally there has been a complaint of a deep, poorly localized, unpleasant pain in the region of the wrist. Similar pain has occasionally been noted in all three types of block, but in the other two it is much less constant than the tingling. It is perhaps curious that tingling was not observed in the cold blocks, since, as Weir Mitchell (1872) has observed, "When we touch or otherwise disturb any part of the trunk of a nerve containing sensory filaments we cause, if the irritation be feeble, subjective referred sensations, such as formication." The absence of tingling in cold blocks and its consistent presence in compression blocks suggests that the physiological mechanisms of the two types of block are probably not identical—a suggestion which we have already put forward on other grounds (Sinclair & Hinshaw, 1951b).

Alterations in the response to sensory stimulation.

1. *Increase in intensity of sensation.*

In the early stages of some of the compression blocks the subjects remarked on what they thought was the introduction of a different set of temperature stimuli, since they now felt the warm stimulus had become much warmer and the cold stimulus colder than at the beginning of the experiment. On investigation it was found that this occasionally occurred in all three types of block, and an attempt was made to compare the sensation produced on the blocked hand with the sensation produced by the same stimulus on the control hand. It was found that in certain cases the warm stimulus was consistently called "warmer" on the blocked hand than on the control, and similarly the cold stimulus appeared colder on the blocked hand than on the control. The stimuli used naturally permitted only a rough assessment to be made, and no measurement of thresholds was attempted. We could not satisfy ourselves that a similar "hyperæsthesia" occurred in modalities other than that of temperature, and even with temperature stimuli the effect often lasted for only a few minutes in the early stages of the block. Brown-Séquard (1857) found an initial hyperæsthesia in the limbs of rabbits deprived of their blood-supply, and Waller (1861) noted a hyperæsthesia at the beginning of nerve blocks induced by cold in man. Again, Goldscheider (1886) concluded that there is a transitory lowering of the pain threshold during the initial stages of the development of surface anæsthesia with cocaine. On the other hand, Dessoir (1892) reported a hypoæsthesia and hypoalgesia in the early stages of compression blocks.

In 1936 Thompson and Kimball reported a drop in the threshold alternating current necessary to evoke sensation from the median, ulnar and radial nerves at the wrist during the first 12 minutes of a compression block of the upper limb. Whether this lowering in threshold has any connection with the subjective increase in the intensity of the sensation occasioned by a constant stimulus is a matter which requires further experiment.

2. *Partial block syndrome.*

At a certain stage of compression cuff blocks, a characteristic picture develops which might be called the partial block syndrome. The response to a touch or pain stimulus is delayed, prolonged and altered in quality, while temperature stimuli evoke a response which is delayed, often prolonged, and may be paradoxical in nature (Sinclair, 1948; Weddell, Sinclair & Feindel, 1948; Sinclair & Hinshaw, 1951a). The fully developed syndrome is perhaps most striking in the modality of pain. Thus,

TABLE IV

ALTERATIONS IN RESPONSES TO STIMULATION: ALL TYPES OF BLOCK

Modality	*Delay*	*Prolongation*	*Alterations in quality*
1. Touch ..	1–2 seconds	2–5 seconds	Feels "dull" and "flat," lacking "overtones." Alterations indistinguishable with different blocking agents.
2. Pain	2–5 seconds*	5–10 seconds	Severe unpleasant affective reaction in cuff-blocks; neither so severe nor so frequently obtained in procaine and cold blocks.
3. Temperature	5–10 seconds	?	Quality changes detectable, specially in cold blocks, but in all blocks overshadowed by almost invariable occurrence of paradoxical sensations.

The figures given are rough approximations only and are not based on exact measurements. They indicate the maximum times encountered. The prolongation of temperature sensations, though probably a real phenomenon, proved difficult to estimate under the conditions of sensory testing.

* Delay longest in compression blocks.

the subject may say of a pinprick, "That is sharp and feels as though it ought to be painful"; perhaps three seconds later his face becomes contorted, his pupil dilates, his hand is involuntarily withdrawn, to the accompaniment of an exclamation, and sweat may appear on his forehead and round his mouth. He now reports a particularly unpleasant experience: a slow swelling burning sting, which may go on and on for several seconds, not very intense, but of a character rendering him fearful of further such experiences. On being pressed, he will find himself unable to compare the sensation with anything he has experienced previously, but may use the analogy of a wasp-sting, or a very fine-pointed, red-hot needle being slowly inserted. It should perhaps be emphasized that the fully developed response does not invariably appear with every stimulus; thus, testing within a given circle at a given time may produce a relatively normal response, a delayed normal response, a prolonged normal response, or a response which is both delayed and prolonged, but not altered in quality. At the same time there may be immediate responses of abnormal quality, prolonged abnormal responses, and finally the characteristic response, delayed, prolonged, and of abnormal quality.

The three components of this sensory syndrome do not always obtrude themselves in the same sequence, nor are they of comparable severity in all modalities (*see* Table IV). Thus, the alteration in quality of touch sensibility is unobtrusive, and may occasionally be missed, whereas no subject experiencing the alteration in the quality of pain in its fully developed form could conceivably overlook it.

A very similar picture is found during the development of both procaine and cold blocks (Table IV), the only major difference being in the relative frequency and severity of the unpleasant response to pinprick (Sinclair & Hinshaw, 1951a and 1951b).

All three sensory symptoms have been the subject of comment in the past, but it is not usual to find them considered in relationship to each other.

A. Delay.

A delay between the application of a stimulus and its recognition has often been noted in compression or pressure blocks (Fabritius & Bermann, 1913; Lewis, Pickering & Rothschild, 1931; Zotterman, 1933; Lewis & Pochin, 1937, 1938; Wortis, Stein & Joliffe, 1942; Sinclair, 1948; Weddell, Sinclair & Feindel, 1948), but in procaine and cold blocks its occurrence and possible significance appear to have escaped any extensive notice.

Zotterman (1933) measured the delay in the perception of stimuli applied to the hairs, and found a steep rise in this time about the 18th minute of a cuff compression. More important, because of the conclusions which have been drawn from them, are the measurements made by Lewis and Pochin (1937) on the delay in pain perception. Impressed by the existence of a double pain response to a single prick on the normal finger, they measured the interval from the stimulus to the beginning of the second response (11 readings in two subjects), and compared these control figures with the interval from the stimulus to the beginning of the delayed response in the same fingers during a cuff block (12 readings in the same subjects between about the 25th and the 30th minute of the blocks). The method of measurement referred to indicates a heat stimulus, but the text and the table imply a pinprick stimulus; no method of measurement is given for the latter. Finding the average of all control readings (about 1·2 seconds) to be almost the same as the average of all the asphyxia readings, they identify the delayed response in asphyxia with the normal "second response." It may be noted, however, that when each subject is considered separately, the figures given show a statistically significant difference in both cases between the control and the asphyxia readings. Further, in these experiments no account is taken of possible alterations in the "reaction time" during asphyxia. In a second paper (Lewis & Pochin, 1938) they state (without offering any additional measurements) that in cuff experiments the pain response "is delayed by at at most 1·5 seconds. . . ." This statement later develops into ". . . for pain the latency rises abruptly from 0·3 to 1·5 seconds, where it remains constant." They contrast this behaviour with that of the cold response, in which they describe a gradually increasing delay to values of up to 10 seconds, and conclude "Clearly the cold response, unlike the pain response, is not carried by two groups of nerve fibres of widely differing conduction rates." In the absence of any figures supporting the asserted "sharp rise in latency" in the case of pain, or the "constant" value attained it is difficult to comment on the validity of these conclusions; the inadequacy of the published measurements needs no emphasis. Wortis, Stein & Joliffe (1942) timed with a stop-watch the delay in the response to pinprick on the foot at intervals during cuff experiments, and obtained values of from 0·9 to 1·7 seconds. They stress the inaccuracy of the method, but support the conclusions of Lewis & Pochin. We have made no attempt to measure the average delay at different stages of compression blocks with any accuracy, but on numerous occasions have recorded (with a stop-watch) delays much in excess of 1·5 seconds, and indeed up to 5 seconds—longer than any fibre currently stigmatized as carrying pain would take to conduct an impulse from the finger to consciousness. (Like those of Lewis & Pochin, these figures do not differentiate between delay due to prolongation of the "reaction time" and delay due to an increase of the time necessary for the evaluation of the nature and quality of the stimulus).

Gasser (1943, 1945) is of the opinion that each modality of sensation is widely distributed throughout the range of fibre sizes, and our own sensory dissociation experiments have led to substantially the same conclusion (Sinclair & Hinshaw, 1951a and 1951b). Further, Magladery, McDougal & Stoll (1950) have recently recorded sensory potentials from the ulnar nerve at the elbow during cuff blocks, and find that "When latencies and amplitudes were plotted against duration of ischemia, there was no break in the smooth curve obtained. It would seem reasonable to assume that this implies a progressive depression and block in the same fibres which initially conducted impulses of high velocity . . . it is possible . . . that ischemia . . . creates a more synchronous volley in the slowly conducting group. . . ."

The findings in procaine and cold blocks are also relevant. The electrophysiological evidence suggests that in procaine blocks the small slowly conducting fibres fail before the large ones, and in cold blocks it appears that the fibres fall out in an order independent of their conduction rates (Bickford, 1939; Sinclair & Hinshaw, 1951b). Yet in both types of block considerable delays have been recorded in the modality of pain, though less frequently than in compression blocks (Table IV). In summary, it may be said that the evidence is at present inadequate to support the identification of the delay between a pain stimulus and "second pain" in normal skin with the delay in asphyxial blocks. It follows that the cause of the asphyxial delay is not necessarily the same as the cause giving rise to the appearance of "second pain," and that it might be profitable to look for a mechanism capable of explaining the occurrence of delay in all modalities in all types of block.

Weddell, Sinclair & Feindel (1948) have already suggested that the delay in the appreciation of painful stimuli in cuff experiments and in clinical conditions such as tabes (Naunyn, 1879; Gad & Goldscheider, 1892; Pochin, 1938; Kendall, 1939; Stein & Wortis, 1941; Kugelberg, 1948; Ashby, 1949, and others) does not necessarily depend on the existence of two discrete groups of fibres conducting at different rates. They point out that the delay could occur, not in the peripheral nerve, but in the synapses of the cord, or in the higher centres, as a result of a simplification of the impulse pattern reaching the central nervous system. Such an explanation would obviously be applicable to all modalities and to all types of block. It is interesting that a similar central factor has been postulated by Stein & von Weizsäcker (1927) to account for the delay in perception of stimuli in clinical cases of hypoæsthesia, and by Kauffmann & Steinhausen (1921) and Piéron (1930) to explain the observation that the reaction time to pain in normal subjects varies with stimulus intensity (Dolley & Cattell, 1894; Kauffmann, 1921; Piéron, 1929; Eichler, 1930; Piéron & Segal, 1938). It is possible that a similar mechanism underlies the variation in the reaction time to pain when different kinds of stimuli are used (Piéron, 1929, 1930, 1931).

Pochin (1938) attempted to show that the delay in pain perception occurring in tabes is the same as the delay in perception of "second pain" in normal subjects, and found the delay to be similar in both instances. The results may, however, be criticized on the grounds that for the tabetics he used a needle with "a 20 g. thrust" and for the normal controls one with "a 2 g. thrust." In any case, as Ashby (1949) points out, there are many recorded instances of much longer delays in tabes, some of which are too long to be accounted for satisfactorily on the basis of delayed nerve conduction alone.

B. Prolongation of response.

Prolongation of the delayed response to a pain stimulus is well recognized in both clinical conditions and nerve blocking experiments (Naunyn, 1879; Stein & von Weizsäcker, 1927; Zotterman, 1933; Lewis & Pochin, 1938; Kendall, 1939; Wortis, Stein & Joliffe, 1942; Weddell, Sinclair & Feindel, 1948; Ashby, 1949, and others). In our experience with nerve blocks, the response to a painful stimulus has frequently lasted as long as 10 seconds, though there is considerable variation, and the time does not appear to increase regularly with the progress of the block. In clinical cases, too, the response can often be of the order of 10 seconds in length (Ashby, 1949). In spite of its frequent occurrence, however, no adequate explanation for the existence of this prolongation has been put forward.

Sinclair (1948) noted a considerable prolongation of the sensation aroused by touch stimuli at a certain stage of progress of compression blocks, and we have since found this to be a fairly consistent feature of procaine and cold blocks also (Table IV). The methods of testing (Sinclair & Hinshaw, 1950) do not permit a categorical statement that the same thing occurs with temperature stimuli, but our impression is that it probably does. At all events, since touch has not yet been associated with two special groups of fibres, we are at liberty to consider the meaning of the phenomenon without the necessity of binding it to the process of conduction in a specific group of nerve fibres, as has hitherto been the case (Lewis & Pochin, 1938; Kendall, 1939, and others). It is perhaps possible that the prolongation of the response is related to the reduction in the number of impulses reaching the central nervous system. Though the analogy must not be pushed too far, it is interesting to speculate whether this type of response is the sensory counterpart of the long "after-discharge" which often occurs in motor reflexes when the stimulus is brief and weak (Sherrington, 1947, p. 33).

C. Alterations in quality of response.

It has already been noted that alterations in quality in asphyxial block experiments are most marked in the modality of pain, and it is to pain that nearly all the attention in the literature is directed (Bastien & Vulpian, 1855; Boeri & di Silvestro, 1899; Fabritius & Bermann, 1913; Lewis, Pickering & Rothschild, 1931; Zotterman, 1933; Lewis & Pochin, 1938; Wortis, Stein & Joliffe, 1942; Weddell, Sinclair & Feindel, 1948; Kellgren & McGowan, 1948, and others). The close resemblance between this type of distortion of pain sensibility and the distortion which occurs in clinical conditions such as nerve regeneration (Head, 1920; Trotter & Davies, 1913; Lanier, 1935, and others), and tabes (Naunyn, 1879; Ashby, 1949, and others), has been noted by many workers, and the matter has been recently discussed by Weddell, Sinclair & Feindel (1948), in the light of observations on the neurohistology of cutaneous scars from which unpleasant pain could be elicited. It was found that the occurrence of an unpleasant response to a needle prick appeared to depend on the existence of a simplified pattern of innervation in the scar, and this was correlated with the occurrence of a very similar type of pain in compression blocks at a stage when the fibres supplying the area tested must be assumed to be falling out randomly and so thinning the pattern of innervation. The idea put forward, which owed much to the suggestions of Symonds (1931), Lanier (1935) and Bishop (1946) was that the unpleasant quality of pain sensibility was correlated with a reduction in the pattern of impulses reaching the central nervous system as a result of a stimulus.

It is interesting, however, and possibly important, that in both procaine and cold blocks the alteration in the quality of pain sensibility is not nearly so marked as it is in compression blocks; indeed, in some cases it was not observed (Tables II and IV), in contrast to its invariable appearance in compression blocks. In all three types of blocks the available evidence suggests that the fibres conducting any one modality from a given area of skin tend to fall out at random, so producing a gradually diminishing pattern of innervation in the skin. A non-specific reduction in neural patterns does not, therefore, appear to be a sufficient explanation for the occurrence of unpleasant pain, and it is probable that a special *kind* of reduction is necessary. We have suggested elsewhere (Sinclair & Hinshaw, 1951a) that the explanation of the differences between compression blocks and procaine or cold blocks may lie in the different affective reaction produced by the activity of large and small fibres. In compression blocks it is possible that the larger fibres are attacked first; these fibres may simply be responsible for the recognition by the subject that he has been pricked, and have little relation to the sensation of pain as it is commonly understood (Trotter & Davies, 1913; Le Dantec, 1919; Piéron, 1923). The remaining smaller fibres, which give the subject the impulse pattern commonly identified by him as "pain," are therefore those which are left to undergo the terminal "thinning out" process which governs the appearance of the unpleasant response. In procaine and cold blocks it is likely that the fibres taking part in this "thinning out" process are, at least in part, the larger fibres subserving "piqûre" (Piéron) and not pain; a reduction in pattern of such a fibre network would indeed give rise to a change in quality, but such a change would not necessarily have the explosive, unpleasant quality of a change in quality aroused within the range of "true" pain.

Any explanation of this sort would, of course, also account for the occurrence of alterations in the quality of other modalities, such as touch (Sinclair, 1948). In all three types of block alterations in the quality of temperature sensibility occur, but they are more difficult to define, and may best be described as giving the impression that the sensation has lost its "brightness" and is descending gradually on the skin from an immense distance; it "soaks in" in a manner quite unlike its normal brisk progress through the skin, and the process is obtrusive. Several subjects were still able to differentiate a warm stimulus from a cold one at a time when both sensations had apparently lost all temperature content (Sinclair & Hinshaw, 1950). No subject could say how this was done, except that the tubes "felt different"; they could not identify either tube as being warm or cold, but could merely distinguish tube A from tube B. Their ability in this respect did not alter when the two tubes were exchanged for others.

D. *Paradoxical temperature sensations.*

Alterations in quality of temperature sensation were in all cases overshadowed by the occurrence of paradoxical sensations, which were characteristic of all three types of block. In the literature, mention of paradoxical temperature sensations during nerve block experiments is practically restricted to chemical blocks (Ponzo, 1909; Schilder, 1913; Goldscheider & Hahn, 1924; Grundig, 1930). Their occurrence in clinical conditions such as nerve injury has also been noted (Barker, 1896), though such reports are relatively uncommon, perhaps because the patient has too often been written off as "unreliable" by the clinician.

Previous workers on the normal skin have tended to stress the phenomenon of paradoxical cold as opposed to paradoxical warm (Alrutz, 1897, 1900, 1906; Thunberg, 1901, and others), but, as Pavlicek & Jenkins (1933) point out, paradoxical warm is commoner than has often been supposed (Goldscheider, 1884; Bazett, McGlone & Brocklehurst, 1930). Nevertheless, Jenkins (1938) using a stimulator surrounded by a variable temperature field, observed paradoxical cold and warm only 27 times in 9,000 stimuli, and in only two out of four subjects, and Gilmer (1942) notes only one report of paradoxical warm from approximately 13,000 cold stimuli. However, in blocking experiments Ponzo (1909) and Schilder (1913) describe paradoxical warm occurring after the injection of stovain into the skin, and Goldscheider & Hahn (1924), who applied various chemicals to the surface of the skin, maintain that paradoxical warm is just as common under these circumstances as paradoxical cold. On the other hand, Grundig (1930) blocked the anterior cutaneous nerve of the thigh with tutocain, and could find no evidence of the occurrence of paradoxical warm from the warm spots in the interval after cold sensibility had "disappeared" while that of warmth was still present. He agrees with Rein (1924) that there is insufficient evidence for the existence of physiological paradoxical warm.

There is thus considerable disagreement in the literature, and it should perhaps be emphasized here that the phenomenon we have described is the appearance of a paradoxical sensation in response to a relatively large stimulus. Blackening the end of the test-tubes used showed that the average area of the tube in contact with the skin was about 50 sq. mm., whereas most of the preceding work has been done with small stimuli designed to investigate the responses of individual cold and warm "spots." One would not, therefore, expect the results to be strictly comparable. With our method of stimulation, there are periods during the development of practically every block when the cold stimulus is called "warm" and the warm stimulus called "cold"; both phenomena are equally common. It is also quite usual to find that in one and the same area at the same time cold is called warm and warm is called cold. This finding would appear to have a number of implications bearing on current theories of paradoxical temperature sensation (Stone & Jenkins, 1940), and on the problem of temperature sensitivity as a whole. One of us (J. R. H.) has a paper on the subject in preparation.

There were additional complications. In all types of block it was quite common to find that, during the failure of temperature sensibility, the subject might confidently call a warm stimulus cold immediately after its application. After a few seconds he might revise his opinion and call it "indifferent," and by about the 10th second he might be equally confident that the stimulus was a warm one. This opinion did not alter if the stimulus was left *in situ* for as much as another 10 seconds. On removal of the stimulus, however, the subject might once again reverse his opinion and maintain that the stimulus was cold after all. Such rapid alterations of subjective judgment were, of course, not invariable, but they were observed in many subjects with both cold and warm stimuli; they did not appear to be a reflection on the reliability of the individual subject.

The effect of repetition of temperature stimuli on the response obtained was also observed (Sinclair & Hinshaw, 1950). Frequently a stimulus which elicited a paradoxical or an indifferent response on first application would be recognized correctly and consistently after it had been applied several times to the same area.

3. *Localizability of stimuli and reference of sensation.*

During the process of extinction of the modalities of touch and temperature in compression blocks, there were occasional spontaneous complaints from the subjects that the stimuli were being applied to a different area, sometimes on the hand, sometimes on the forearm or elbow. These "references" of touch and temperature sensations were inconstant, and were not noticed in either procaine or cold blocks. The referred sensation usually had an altered quality and felt, in the case of touch, like a prolonged "tickle." Often stimulation of the same area a few minutes later would still produce reference to the same situation. It is perhaps curious that reference of pain was not observed, since pain is fairly frequently referred from the normal skin (Sinclair, 1949). Further, painful stimuli remained fairly well localized even at the stage of unpleasant responses, whereas touch and temperature stimuli were often poorly localized just before they disappeared. Little can as yet be said regarding the reason for the occurrence of reference in compression blocks; it may perhaps be connected with the progressive reduction in sensibility of the stimulus area, which would render a referred sensation more noticeable than under normal circumstances, when the impression from the stimulus area would be so overwhelming as to exclude from attention the feeble referred sensation. This sort of explanation might also account for the lack of reference of pain at a state when the local response is characteristically unpleasant.

4. *Extinction of sensibility.*

The main object of carrying out the blocks reported in this paper was to throw light on the factors determining the order in which the various sensory modalities apparently disappeared in such experiments. This work has been reported elsewhere (Sinclair & Hinshaw, 1950, 1951a and 1951b), but a number of points of interest may be stressed.

The extinction of a given modality is a gradually progressive and continuous process; when one spot ceases to respond, another close by may continue to give responses for some time, and the pattern formed by such responsive spots gradually becomes thinned out and disappears. When responses to a given stimulus can no longer be obtained, a more intense stimulus—e.g. a heavier nylon thread—will still produce responses, and when these in turn disappear, a further increase in stimulus intensity will prolong the extinction time still further, and so on. Repetition of touch stimuli, individually ineffective, often leads to a response, probably owing to some sort of central summation process (*cf.* Achelis, 1936). It is essential, therefore, in this type of work, to specify exactly the nature of the stimulus and its mode of application before it becomes possible to say that "touch" disappears before "pain" and other statements of the kind. Too little regard for this necessity has, in the past, lead to grossly contradictory results, since by manipulation of the stimuli used it is often possible to record several different orders of sensory loss in the same experiment. Further, the end-points adopted must be the subject of close definition. In this work we used end-points of the "50 per cent. mortality" variety, in the belief that the type of response curve under these circumstances probably corresponds to the familiar dosage/mortality curves of probit analysis. Unfortunately the rapid progress of the blocks did not permit us to base our 50 per cent. mortality on more than 6 or 8 successive stimuli. Nevertheless, the endpoint so defined is capable of fairly accurate reproduction by different observers, and in view of the many other factors involved, is probably accurate enough for the purpose. Thompson, *et al.* (1934), dealing with a similar problem, used a nil response from three *consecutive* stimuli as a criterion of a stimulus being below threshold, and justify their method mathematically; it is probable that this system is preferable.

We have no measure of the absolute energies involved in the stimuli used in this investigation, and it is likely that they differ grossly from one modality to another. For this reason, the end-point for one modality is almost certainly not comparable to that of another, in terms of the nerve impulses aroused by each stimulus. Again, the results obtained in one area of skin within the territory of a given nerve can be different from those in another area, and the behaviour of one nerve territory can be grossly different from that of another. In short, it appears unlikely that it will ever be possible to say, in absolute terms and without numerous qualifications, that in nerve blocks in man one modality fails before another. Any inferences drawn from such statements, such as a relation between fibre size and sensory modality (Gasser & Erlanger, 1929), are therefore open to question.

5. *Spontaneous temporary recovery.*

In the cold blocks in this series it was frequently observed that there were occasional spontaneous remissions in the march of the sensory paralysis, during which the sensibility of the part might recover to a condition indistinguishable from normal by the testing methods at our disposal. Several such recoveries might occur during the course of one block, without any alteration in the temperature of the freezing mixture. This type of irregular progress of paralysis in cold blocks appears to have been first noticed by Waller (1861) who noted that in some cases "we have hyperæsthesia and analgesia of the nerve recurring several times before the supervention of the stage of anæsthesia." Lewis (1941) has described a related type of irregular recovery in sympathetic fibres under similar circumstances.

Perhaps more important, because of its bearing on the nature of the modality of warm, is another type of sensory recovery affecting only this modality and occurring both in cold blocks (Sinclair & Hinshaw, 1951b) and in compression blocks. In this case the reappearance of an ability to identify warm stimuli correctly and consistently some time after this ability had been extinguished appears to be connected with the onset of vasomotor paralysis. In the cold blocks, at any rate, it is strongly correlated with the appearance of a rise in skin temperature, and during the period of recovery it may disappear temporarily with the recovery of vasomotor control (*see* p. 67). In procaine blocks, vasomotor paralysis occurs too early (it is often the first sign) for the connection to be investigated.

It is also interesting that there is, in both cold and compression blocks, frequently a sensation that the whole hand is warm and glowing, at a stage when no responses occur to warm stimuli applied to the skin. Lewis, Pickering & Rothschild (1931) note that in compression blocks, as late as the 35th minute, the tip of a finger dipped in water at 40° C. "feels an undiminished and glowing warmth."

There have been numerous attempts to correlate the sensations of warmth and cold with the activity of blood vessels and the sympathetic nervous system generally (Nafe & Wagoner, 1936; Hyndman & Wolkin, 1941; Gilmer, 1942, and others) and certainly no satisfactory specific end-organ has ever been produced to bear out the theory suggested by v. Frey (Dallenbach, 1927, 1929; Kantor, 1947, and many others). Nafe & Wagoner (1937) found that in compression blocks the failure of cold sensibility coincides with the failure of the blood-vessels in the hand to respond to a cold stimulus given to the contralateral hand. Their conclusions do not appear justified by the few observations presented, but nevertheless it appears that work of this kind could throw some light on temperature sensibility. The findings in the present series of blocks have not yet been followed up, but they suggest a possible line of enquiry.

Sensory hallucinations.

These were confined to compression blocks, and related to the position of the elbow. Seven subjects, at a time when all sensation in the hand except that of pain had been extinguished, spontaneously remarked on the fact that they felt that the forearm had risen from its dependent position and now lay roughly at right-angles to the arm. All were profoundly surprised to find that in fact the forearm had not moved. After they had satisfied themselves of this, the hallucination usually disappeared.

Process of recovery.

1. *Sensations induced by the recovery process.*

No particular sensation could be associated with the recovery process in procaine blocks, but in a number of cold blocks recovery was accompanied by pain in the cooled region of the elbow, with or without nausea and faintness increased by handling the territory of the ulnar nerve. This may be due to the liberation and rapid dissemination of a histamine-like substance from the damaged tissues of the elbow, and it was found that if the limb was kept quite still and not interfered with, these symptoms did not occur (Sinclair & Hinshaw, 1951b). Pain referred to the hand was not observed.

On a release of a compression block, a series of fleeting thermal impressions was often felt, such as a "wave" of warmth appearing to "hit" the hand, succeeded by a transient feeling of cold lasting a few seconds. This was in turn followed by a warm "glow." These temperature sensations were inconstant, and have not been investigated.

The existence of "formication" during the recovery from compression blocks has been noted in the literature for some time (e.g. Carpenter, 1847), but it was not until 1931 that Lewis, Pickering and Rothschild investigated its nature and cause. They concluded that it arose in the nerve fibres directly underneath the investing cuff, and suggested that it was "an affair of the tactile nerves." Weddell & Sinclair (1947) described several elements in the sensation complex which succeeds release of compression, and concluded that the tingling or "pricking" component was largely due to the stimulation of pain endings in the hand by the removal from their vicinity of a metabolite which had accumulated there during the period of compression.

Merrington & Nathan (1949), as a result of similar experiments, concluded that the impulses arose in the nerves and not in the endings, and that they were carried in fibres normally subserving touch, pressure and movement.

This conflict regarding the quality of the sensation produced in this way results largely from the observation of patients with lesions of the nervous system, which is necessarily an unreliable basis for work on sensation. One of us (D. S.) has investigated release paræsthesiæ in the same 28 patients used for the investigation of tingling during compression. The evidence so accumulated suggests, in contrast to the findings of Merrington & Nathan, that the impulses of release paræsthesiæ travel, at least as far as the thalamus, along pathways normally conveying pain; when these pathways are intact, the sensation is felt, and when they are damaged, the sensation is diminished or absent. In the cases with cerebral lesions, defects in the appreciation of pain were correlated with defective appreciation of the paræsthesiæ, while defects in the appreciation of touch or pressure produced little or no effect on the intensity, duration or quality of the sensation.

The most recent work is that of Magladery, McDougall & Stoll (1950), who recorded the impulses from the ulnar nerve at the elbow during and after compression blocks. Their evidence indicates that the impulses arise distally in the limb, probably at the endings, and they have shown that the first fibres to recover (about the time of onset of formication) are those of high conduction velocity. They conclude that pain pathways are not likely to be involved.

These difficulties may perhaps be resolved by a consideration of the introspective findings. When a subject is asked to describe the sensation he experiences under these circumstances, one of two replies is almost invariably made; either it is called "pins and needles" or it is described as a "pricking" sensation. The implication of "pricking" is thus invariable, though the sensation may be described as painful by one subject and not painful by another. May it not therefore be that the sensation is closely connected to the sensation of "piqûre" which Piéron and others have stoutly maintained must be separated from pain? It would occasion no surprise to find "piqûre" travelling in the fast fibres, and indeed it could be regarded as the response typically aroused by the largest fibres in a spectrum whose slower members give responses which are characteristically recognized as painful. The pathways traversed by the sensation of "piqûre" may well be multiple, and may differ from patient to patient, but it is far more likely that confusion arises owing to the difficulty of patients in interpreting and naming the sensations which they experience.

2. *Response to sensory stimuli during recovery.*

In both cold and compression blocks the process of sensory recovery was usually extremely difficult to follow because of the risk of causing syncope in the cold blocks and the constant presence of paræsthesiæ in the compression blocks, so that the procaine blocks afford the only really satisfactory data on recovery. In the literature, records of the mode of recovery in compression blocks are scanty, though Raffel (1936) has reported two experiments on the leg. Recovery in cold blocks has been studied by Bickford (1939) and in procaine blocks by Heinbecker, Bishop & O'Leary (1934). With procaine blocks there is a difficulty not experienced in the other two types, namely, that the process of recovery is self-initiating, and no control of its onset is possible. Nevertheless, enough information has been accumulated in all three types of block to make certain general statements permissible.

Three stages could be recognized in the recovery process; the first being the ability to recognize the sensory stimuli consistently. This stage is reached quickest in compression blocks and slowest in procaine blocks; the various modalities do not always recover in the same order as they disappeared. In the cold blocks it was observed that the modality of warm, which was often present at the end of the block, disappeared again in several instances at the time of recovery of the sympathetic fibres, and only later returned permanently.

The next stage was reached when no subjective difference could be detected between the sensations produced by the standard stimuli on the affected and on the control hand; this took considerably longer than the first stage in all types of block. Though no difference could be detected by the crude stimuli employed, at this time there was still a subjective difference between one hand and another, and it was not until perhaps some hours later that the last stage—that of complete subjective recovery—was reached. The difference between the two hands, which could not be satisfactorily described by any subject, could be emphasized by stimuli such as stroking the hand with the other hand. In all types of block the last modality to recover *completely* was that of touch, irrespective of the order in which touch sensibility returned when the standard stimuli were used. This finding may be correlated with the observation that during the development of the blocking process the first modality to be subjectively disturbed was in nearly every case that of touch, irrespective of the type of blocking agent and of the order in which the modalities eventually disappeared. We have previously (Sinclair & Hinshaw, 1951a) drawn attention to this discrepancy and suggested that a small deficiency in the modality of touch, a modality of which we make use so constantly, may be more readily detected than similar deficiencies in the other modalities, which we do not so often experience.

IV

General Discussion

Bastien & Vulpian (1855), who were among the first to conduct systematic investigations into the phenomena accompanying nerve blocks caused by the application of pressure, wrote:—"En résumé, une première et attentive exploration nous a fait voir que l'étude des effets de la compression des nerfs, de ces phénomènes très-connus, mais peu analysés jusqu'à présent, était une mine très riche et pouvait être féconde en résultats applicables à la physiologie et à la pathologie du système nerveux." The position is still very similar. The "mine" is still unexhausted, and the results still wait to be worked out. Even the phenomena themselves are by no means so generally known as was assumed nearly a hundred years ago, and it is partly for this reason that misconceptions have risen, such as the correlation of the order of sensory loss in nerve blocks in man with the way in which fibres fall out according to their size in animal experiments. In the human experiments insufficient attention has often been paid to controlling the mode of stimulation and the area stimulated and to the possibility of extensive individual variation.

One of the most interesting features of nerve blocks in general is the development of what we have called the "partial block syndrome"—i.e. the association of delay, prolongation of response and altered quality of response. It does not seem improbable that all three features of this syndrome may arise from the same basic cause—a deficiency (perhaps of a special kind) in the impulse pattern reaching the central nervous system as a result of the application of a stimulus. Such a hypothesis might well be applicable to similar phenomena known to occur in clinical lesions of the nervous system.

Similarly, the further elucidation of the precise nature of the sensations produced by the development and recession of the blocking process might have a direct application to problems connected with the occurrence or paræsthesiæ in nerve injuries, etc.

The phenomena in relation to temperature sensibility also merit further investigation. The paradoxical sensations, which are produced with difficulty from the normal skin, but which are so readily elicited by the technique described, may well throw light on the nature of temperature sensibility in general, while the possibility of a kind of relationship between warm sensibility and the activity of the sympathetic nervous system is also of importance. When so little is known for certain about temperature sensation, additional lines of attacking the problem have a disproportionate significance.

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VARIATIONS IN REFLEX BLINK-RATE DURING VISUAL-MOTOR TASKS

BY

G. C. DREW

(Department of Psychology, University of Bristol)

The influence of the performance of various tasks on the reflex blink rate has been investigated repeatedly, but the results obtained have been somewhat contradictory. More recently, it has been suggested that the reflex blink rate may be a considerable factor in the accuracy with which visual-motor tasks are performed. The aim of the present experiment was to attempt to obtain further evidence on both these questions.

The experiment was carried out in two parts. The main part was done in the laboratory, while a validating experiment was carried out on the road. In the laboratory, subjects were required to steer a pencil along a moving track which varied in difficulty. The difficulty of the response was varied by using a direct control on some trials and a velocity control on others. Blink rate, errors, and control measurements were recorded throughout. In the road experiment, cine films were taken of the driver's eyes while driving in heavy traffic and in open country.

The results of both experiments show:

(*a*) That there are marked individual differences in blink rate, the relative order of which is maintained in spite of variations in the actual blink rate.

(*b*) That there is no relationship between the accuracy with which a particular individual carries out a task of this sort and his blink rate. The rapid "blinker" is no more and no less likely to be accurate than the infrequent "blinker."

(*c*) That the actual blink rate for all individuals varies inversely with the difficulty of the task and the amount of control movement necessary. The blink rate decreases as the necessity for detailed visual control of movement increases. Blink rates when driving a car in heavy traffic, or when steering along an oscillating track, are considerably lower than when driving in open country or when following a straight track.

(*d*) That the adjustment of the blink rate to the difficulty of the task is achieved not only by an alteration in overall blink rate, but also by a change in the distribution of blinking. The blink rate is approximately constant under constant conditions, but when the task is varying in difficulty, blinking occurs just before and just after periods of maximum difficulty, but is completely inhibited during the periods of maximum difficulty itself.

I

INTRODUCTION

THE relationship of the reflex, involuntary blink rate to the type of activity in which the individual is engaged has been a subject of controversy for some years. Ponder and Kennedy (1928) attempted to discover the effective stimulus for the involuntary blink and were able to show that this was not provided by corneal or conjunctival stimulation, by the perception of light, or by tensions in the muscles controlling eye movements. They concluded that there appears to be no afferent path for this response, and that the response probably has a central rather than a peripheral origin. They suggested that the involuntary blink is caused by periodic discharges down the 7th nerve, the impulses originating in the basal nuclei. In the course of their experiments, they demonstrated that the blink rate varies between individuals, but is relatively constant for a given individual if the conditions are kept constant. Some changes of condition, such as a change from light to darkness, did not affect the blink rate of their subjects significantly, while other conditions, such as changing from reading to conversation, from far to near fixation, sudden anger, and the subjection of witnesses in a court of law to cross-examination, produced marked changes, usually in the direction of increasing the blink rate. From comparative evidence supplied by Blount (1928), they suggest that in early life "blink movements develop either in connection with attempts at action on the part of the child, or with the perception of unexpected sounds or objects in its environment," and conclude that the blink movements are clearly linked up with mental development, so that "there is every reason to expect the close relationship which exists between the movements and the mental operations of later life."

That there is a marked and fairly consistent variation in blink rate between different inviduals has been demonstrated conclusively, and there appears to be little doubt that the blink rate for a given individual varies from time to time. The nature of this individual variation, however, does not seem to be at all clearly known. Peterson and Allison (1931) found that reading, cancelling letters, near and far fixation gave markedly different blink rates. Telford and Thompson (1933) recorded an increasing blink rate comparing reading, conversation and mental arithmetic in that order. Similar results were obtained by Walter (1941). Macpherson (1943), comparing blink rates using as tasks counting rows of dots, picking up aircraft silhouettes, plotting graphs and reading, concluded that rate of blinking is negatively correlated with the ease of doing a task.

While Luckiesh (1944 *a*) is sceptical of the use of the blink rate to equate different kinds of task, he and his collaborators (Luckiesh, 1944 *b*, 1946, 1947; Luckiesh & Moss, 1942; Luckiesh, Guth & Eastman, 1947), have claimed changes in the blink rate, on a given task, with changes in the relative ease of seeing. Using reading at a normal speed as a task, they have found increases in the blink rate during one hour, the rate of increase varying with the efficiency of illumination, very dim or very bright illuminations producing greater increases than medium illuminations. Luckiesh, in fact, has used the relative increase in blink rate as a means of determining the optimum lighting conditions for a given activity. Increases in blink rate during prolonged periods of visual activity have also been reported by Carpenter (1948), Tinker (1945, 1949) and Hoffman (1946).

Bitterman, and Bitterman and Soloway have carried out a series of investigations into the change in blink rate with the difficulty of the task, and have failed to find any consistent variations. (Bitterman, 1945, 1946, 1947, 1949; Bitterman and Soloway, 1946 *a*, *b*). In these experiments, the Minnesota Clerical Test has been used as reading material, which, as Luckiesh has pointed out, does introduce speed and accuracy factors not present in his own work. The difficulty of the task has been varied by introducing glare conditions, or auditory distractions. His results have lead Bitterman to claim that the blink rate is too unreliable to be used either as a measure of fatigue or of the difficulty of the task. Tinker (1946) found no significant difference in blink rate in subjects engaged in reading material of unequal difficulty as measured by speed of reading. Brozek, Simonson & Keys found no differential increase in blink rate during one hour, using a task analogous to conveyor-belt inspection, under widely differing illuminations, but did find increased overall average rates under low and high illuminations as compared with medium illuminations.

Hall (1945), investigating the blink rates during conversation and during reading aloud, found a marked reduction when reading aloud. He found, moreover, that blinking, when reading aloud, is not uniformly distributed but occurs at punctuation marks and when turning over pages. Printing the sentences in vertical columns reduced the blink rate further, and especially at punctuation marks, blinks being now concentrated during the time at which pages were turned.

Lawson (1948), working from figures derived both from his own observations and those reported by Duke-Elder (1942), summarized the extent of individual variation in blink rate on a constant task, and calculated the period of blackout and of impaired vision arising either from lid closure or from saccadic eye movements during blinking. He calculated that the majority of people are completely blacked out for 10 per cent. of their waking time, and that the percentage is considerably increased for the more frequent blinkers. He found, in fact, that 21 per cent. of the population tested had an inter-blink period of less than 1·6 seconds; and 8 per cent. less than 1·2 seconds and 0·5 per cent. less than 0·8 seconds. From this, he argued that people with high blink rates, with a consequent high blackout time, will be less efficient in performing visual tasks and will be more likely to be involved in accidents, for example, than people with low blink rates.

The experiment reported here was designed to test the relationship of accuracy on a visual-motor task to blink rate, and the variations in blink rate during periods of differing difficulty on the same task, in comparison with previous work where the comparison of blink rate with difficulty has been made using different tasks.

II

APPARATUS AND METHOD

The investigation was carried out in two parts, the first in the laboratory and the second in a car being driven along a road. The purpose of the field study was largely to check the validity of the laboratory study under road conditions.

A. Laboratory Study.

It was decided that subjects should be set a task which required them to respond to a visual stimulus which placed varying demands on the amount and speed of response required to follow it. If the rate of blinking is related to the amount of visual control of responses, it appeared desirable that the task set should meet the following criteria:—

(i) That the visual stimulus should remain constant at some periods and should change relatively continuously at others.

(ii) That the responses required should be related to the visual stimulus, requiring little or no response to a constant stimulus, and a continuous response to a changing stimulus.

(iii) That the response should be one which would allow the degree to which it followed the stimulus to be scored.

(iv) That the subjects should be unaware that the experiment was concerned with blink-rates. This implied that the task must be such that a plausible alternative explanation could be given, and that the recording of actual blinking should be unobtrusive.

To meet these requirements, a visual tracking task was designed in which the subjects were required to steer, by remote controls, a pencil along a path which at times was straight and at times oscillated from side to side in such a way as to require rapid and fairly vigorous responses to follow it.

The track was provided by drawing a black line, ⅛ inch wide, on a band of paper 6 inches wide. The line alternated between being drawn straight and in the middle of the paper, and oscillating irregularly across it. A stencil was used in drawing the lines to ensure uniformity for different subjects. Fig. 1 shows part of the first straight piece and of the first oscillating section. The paper band with the line drawn on it was mounted on a standard "long" kymograph. A screen with a window cut in it was mounted between the subject and the kymograph. When the kymograph was rotated the black line moved horizontally past the window from the subject's right to left, at a speed of 100 m.m.s per second. The window in the screen was so arranged that the front of the moving line, to which he was responding, was at the extreme left-hand edge, and the window was of such a size as to allow him vision of any part of the track during 30 seconds before he need respond to it. He was thus able to have anticipatory information on the kind of response he would have to make in the fairly near future.

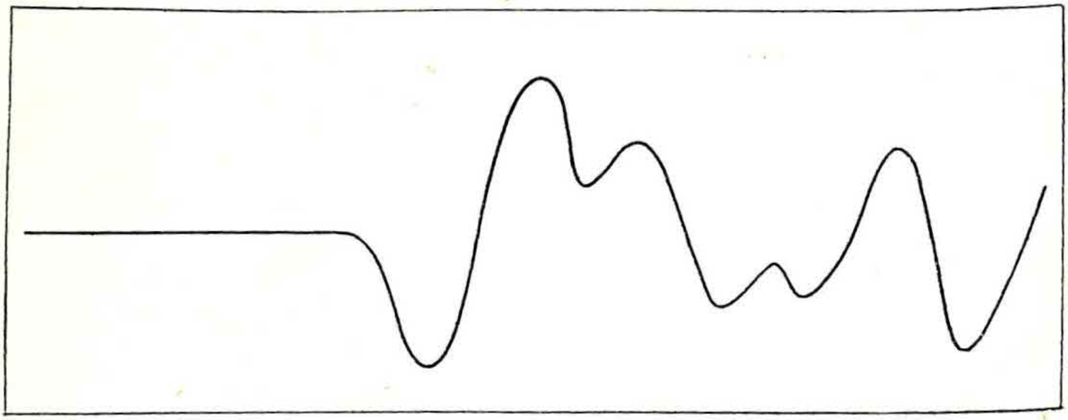

FIGURE 1

SAMPLE OF TRACK

A pencil, fitted with four different coloured leads, was mounted on a slide, fixed to the kymograph stand, in such a way that it slid easily across the paper band in a vertical direction. Only the writing point and about 2 inches of the pencil were visible to the subject through the window in the screen.

The subject controlled the movements of the pencil by means of an apparatus modified from a Craik Triple Tester. This apparatus consisted of a wooden control wheel, 6 inches in diameter, mounted with its axle horizontal. Rotation of this wheel by the subject rotated an axle on which were mounted two rack-and-pinion mechanisms. One of these was connected directly to a sliding arm mounted immediately behind the control wheel. The pencil was pulled vertically up the face of the kymograph drum by fishing line tied to this sliding arm, and was pulled downwards by return weights. Thus, with this rack and pinion engaged, the subject had direct control of the movements of the pencil, the speed and direction of which corresponded to the speed and direction of the movements of the control wheel.

A small gearbox allowed the experimenter to disengage this direct drive and to operate instead the second rack and pinion which substituted a velocity control for a direct control. Movements of the control wheel when this second mechanism was engaged moved a friction wheel across the surface of a rotating brass plate. This plate was driven by a synchronous motor. Movement of the sliding arm, and thus of the pencil, was now dependent upon the speed and direction of rotation of the friction wheel. If the friction wheel was kept in the centre of the rotating brass plate, it, and thus the pencil, remained stationary. Once off centre, however, the friction wheel rotated, the speed and direction of rotation being dependent on the direction and distance off centre. The subject, by moving his control wheel, imparted a movement to the pencil, of which he could control the direction and velocity. Thus a given movement of the control wheel would start the pencil moving either vertically upwards or downwards at a given velocity. The control wheel had then to be returned to its central position in order to stop the movement. The position of the friction wheel on the rotating plate could only be determined by observing the movement of the pencil. The mechanism was hidden from the subject and the control wheel had no tendency to centralize itself. Movements of the control wheel were recorded on a modified revolution counter. The gears were so arranged that, in both direct and velocity control positions, rotating the control wheel to the right moved the pencil upwards and to the left, downwards.

The subject's task was, whether given direct or velocity control, so to operate the control wheel that the pencil point remained on the black line on the moving paper. This meant keeping the pencil stationary when following the straight lines, and moving it up and down at different speeds to follow the oscillating line. In effect, the subject appeared to be steering the pencil along a track which was sometimes straight and sometimes very winding. The slopes of the oscillations of the track varied so that at times a slow, gentle movement was required to follow, while at others the pencil had to be moved as rapidly as the apparatus would allow. The pencil point drew the actual track made on the paper, thus allowing the accuracy of tracking to be secured.

The subject was seated comfortably at a table 8 feet away from the track he was to follow. The track was illuminated by an ordinary ceiling mounting of a 60-watt fluorescent tube. This lighting proved adequate, the ⅛-inch black line on white paper being perfectly clearly visible, without any glare being experienced. The control wheel was mounted on the table, central to his body, and in a parallel plane, and at a convenient height. The subject had an uninterrupted view of the track to be followed over the top of the control wheel.

It has been mentioned above that it appeared desirable that the subjects should be unaware of the fact that their blink rate was being investigated. This precluded any measurement of blink rate by electromyographic techniques. It was accordingly decided to use observers to record the subjects' blinks. An observer was therefore stationed behind the screen and slightly to one side of the window, in such a position that he could see the subject clearly while not himself being obtrusive. All experimental instructions to the subject were given from this position, and the subjects appeared to accept the view that this was a convenient position from which to observe the experiment, from which to change the kymograph tracks and generally to control the experiment. All switches were mounted in this position, out of sight of the subject, and included a key which was depressed each time the subject blinked. Subjects appeared to accept the experimenter in such a position without suspecting his real function there. The experimenter, then, depressed a key each time the subject blinked. This activated an electro-magnetic pen which recorded on the paper on which the stimulus track was drawn behind the screen and out of the subject's sight. This pen was mounted immediately above the moving pencil, so that the approximate time of the blink relative to the particular part of the track being followed could be ascertained. The key and pen were damped with rubber buffers to reduce noise. Residual noise was effectively hidden by the noise of the electric motor.

This method of recording blinks has two main disadvantages. The first of these is that the observer himself blinks and so may miss some of his subject's blinks, and the second is that it is impossible to time the blinks completely accurately, because of the observer's reaction time. A preliminary experiment was carried out to find out whether the error introduced by the observer himself blinking was likely to be significant for the results obtained. To do this, subjects were given a task identical with that of the main experiment. Blinks were recorded by three different observers, additional to the experimenter, all stationed behind the screen. Four subjects were used, the blinks of each being recorded each minute for 30 minutes. It was found that the variation between observers was very small, showing a mean variation of ± 2 per cent. from the total number recorded by the experimenter. No signicant differences in the distribution of blinks in time were obtained from the records of the four observers. It was concluded, therefore, that though the total number of blinks recorded could not be regarded as completely accurate, no significant bias was introduced into the research by such a recording procedure. During the experiment proper, two observers were used. The first gave the experimental instructions, started the apparatus and recorded blink rate during the experiment. The second recorded, at the end of each minute, the reading of the revolution counter attached, by means of fishing line, to the control wheel. To give a "base-line" of blink rate when no visual task was being carried out, the second observer recorded the blink-rate, using a hand counter, each minute during the period in which the subject was listening to the experimental instructions, but before any practice period started.

The subject, when seated at the control table, was told that the experiment was designed to throw light on the relative efficiency of direct and velocity control mechanisms in tracking, a problem which has been the subject of considerable research. All the subjects, who were undergraduates, both men and women and all aged between 19–25, and none of whom had acted as subject in an experiment previously, accepted this without question. It was quite clear, both from their behaviour, and from questioning when the experiment was completed, that none of them ever suspected that their blink rate was being investigated, or had any doubts of the authenticity of the reason given them.

When the subject was seated at the table, he was given the experimental instructions, which included the alleged reasons for the experiment, as outlined above, and the statement that he was required to steer the pencil along the moving track by operating the control wheel. It was explained to him that the control wheel would be varied so that on some trials he would have direct control of the movements of the pencil while at others he would be able to control the direction and velocity of its movements. During this period, the second observer recorded minute by minute each blink on a hand recorder. The kymograph was then switched on to demonstrate the kind of track which would have to be followed, and the speed at which it moved. With the track then stationary, the subject was familiarized with the movements of the pencil relative to his movements of the control wheel. Using, first, the direct control, the subject was asked to move the pencil up and down at varying speeds across the face of the track until he reported that he understood the mechanism and felt he could control the pencil movements adequately. The apparatus was then changed to velocity control, and the same procedure followed. When the subject appeared confident that he was familiar, both with the stimulus he was required to follow, and with the movements necessary to steer the pencil, the experiment proper began.

A new, but identical, track was placed on the kymograph and the pencil centralized as the drum was rotated to the starting position, so that the subject started each trial with the pencil on the track. One complete revolution of the drum completed each trial. This took exactly eight minutes. The track, during these eight minutes, was designed so that straight and oscillating portions followed one another as under:

1st minute	straight
2nd minute	oscillating
3rd and 4th minutes	straight
5th and 6th minutes	oscillating
7th minute	straight
8th minute	oscillating

Blinks were recorded continuously throughout each eight-minute trial.

Four trials were given in all at each experimental period, two with direct control, and two with velocity control. The 1st and 4th trials were given using direct control, and the 2nd and 3rd with velocity control. The tracing made by the pencil was too faint to be visible to the subject, though it was quite legible for scoring purposes. It was found that scoring became difficult if more than two tracings were made on one track. A clean track was therefore mounted on the kymograph between the 2nd and 3rd trials. This meant that a pause of a few minutes duration was interposed between each trial; between the 1st and 2nd while the apparatus was being changed from direct to velocity control, between the 2nd and 3rd while the track was changed, and between the 3rd and 4th while changing back to direct control. The amount of movement of the control wheel was recorded on a modified revolution counter, and was noted each minute throughout each trial.

The accuracy of tracking was computed by measuring with a planimeter, the area between the edge of the true track and the pencil tracing. This gave an error score, representing an integration of the amount and length of time of displacement from the true track, with all errors taken as positive.

The number and distribution of blinks, the amount of movement of the control wheel per minute, and the tracking errors were recorded for each subject for:

8 minutes following a straight track using direct control
8 minutes following a straight track using velocity control
8 minutes following an oscillating track using direct control
8 minutes following an oscillating track using a velocity control

and as a "base-line" the number of blinks per minute before any visual-motor task was attempted.

The experiment thus gave varying degrees of complexity in the visual stimulus, and varying degrees of complexity in the required motor responses.

III

RESULTS

(a) *Relative difficulty of the different conditions.*

It is clear that the four conditions of this experiment represent different complexities of task in responding to a visual stimulus. Following a straight line using direct control was extremely simple; nothing, in fact, needed to be done except to steer the pencil on to the track initially and leave it alone. Following a straight line using a velocity control proved more difficult, since this entailed steering the pencil to the track and finding the one position of the friction wheel on the rotating disc at which the pencil would remain stationary. Slight misplacements to one side or the other resulted in a steady drift of the pencil away from the track. Consequently, although the visual stimulus was still simple, a more complex response was needed to perform the task accurately. The oscillating track provided a task when direct control was used, which placed a premium on rapid and accurate responses. The relationship between stimulus and response was regarded by the subjects as simple. The difficulty of this section is due to the fact that the control wheel had to be turned very rapidly, and with frequent changes of rate and direction to keep the pencil on the track. It is what might be called the "pressure" of the response which causes difficulty here.

Steering the pencil along an oscillating track using a velocity control proved to be very difficult. Fewer, and in general, slower responses were required than when using direct control, though quite rapid movements were necessary to follow major changes of direction of the track. The difficulty here lay not so much in the rate of response as in its complexity. The response required to follow the track moving upwards, for example, at 60° to the horizontal using a direct control was a rapid and continuous rotation of the control wheel to the right. The difficulty lay in moving the wheel sufficiently quickly and at a constant rate. To follow the same track using a velocity control demanded that the control wheel should be moved to the right to a particular position. This position could only be determined by watching the movement of the pencil relative to the track and adjusting the control wheel accordingly. As the track frequently changed its direction, very little time was available for such adjustments. There is some evidence that subjects found this so difficult that they were satisfied with only approximate accuracy. If the pencil was moving in the right direction, and at approximately the correct rate, they tended to leave it alone even though it might be running parallel to and not on the track. This was not found when direct control was used. It appears probable, therefore, that the amounts of error made when using velocity control and direct control are not strictly comparable as measures of the difficulty of the task. In reporting the results in more detail, therefore, these two will be kept separate.

The Mean Error of all subjects for each successive minute is shown in Table I. It will be noted that the distribution of errors is broadly similar whether direct or velocity control is used, but with a greater number of errors being made using a velocity control. As would be expected, following an oscillating track gives a greater error score than following a straight one, with both types of control. No measurable errors are made in following a straight track using a direct control.

Table II shows the distribution of Mean Control Wheel Movements for all subjects in each successive minute. The units in which the amount of movement is plotted are the number of feet of movement of the circumference of the control wheel. Since the control wheel could only be manipulated by grasping its circumference, this unit represents the distance through which the subject's hands were moved. Comparison of Tables I and II reveals a similar pattern of score distribution for errors and movements. Less movement is required, however, to follow an oscillating track using velocity control, though more errors are made.

TABLE I
MEAN ERROR PER MINUTE
SQUARE INCHES DEVIATION FROM TRACK

Minute	*Direct control*		*Velocity control*	
	Straight track	*Oscillating track*	*Straight track*	*Oscillating track*
1	0·00		0·90	
2		2·00		3·20
3	0·00		0·80	
4	0·00		0·62	
5		1·22		2·80
6		0·95		2·32
7	0·00		0·92	
8		1·20		3·00

TABLE II
MEAN CONTROL WHEEL MOVEMENTS
FEET OF MOVEMENT PER MINUTE

Minute	*Direct control*		*Velocity control*	
	Straight track	*Oscillating track*	*Straight track*	*Oscillating track*
1	1·60		10·50	
2		36·54		32·50
3	1·00		7·22	
4	0·52		3·00	
5		34·22		32·00
6		26·00		23·60
7	0·75		5·52	
8		44·00		38·00

(b) *Variations in reflex blink rate.*

The subjects used in this experiment showed a considerable variation in blink rate during the pre-experimental period, though the figures are slightly lower than would be expected from Lawson's figures. The mean blink rate under these conditions was 14·0 per minute, with a range of from 3·0 per minute to 28·0 per minute. The individual readings are given in column 1, Table III. From this it will be seen that considerable individual differences in blink rate exist even in so small a sample of subjects.

The crucial part of this experiment is the variation in blink rate under the different conditions used, which can be interpreted as differing from one another in the "loading" of both stimulus and response. Table III shows the blink rate per minute for each subject under the different conditions. The frequencies given for the four experimental conditions represent the average frequency per minute for each individual for the eight minutes spent under each condition.

TABLE III

BLINK RATE UNDER DIFFERENT CONDITIONS

Subject	*"Resting"*	*Direct control*		*Velocity control*	
		Straight	*Oscillating*	*Straight*	*Oscillating*
1 ..	28	24·0	15·0	19·0	19·3
2 ..	24	19·5	6·6	16·7	9·0
3 ..	23	8·2	1·9	2·7	1·1
4 ..	18	8·5	1·5	6·6	2·0
5 ..	17	12·1	1·1	12·0	1·9
6 ..	11	8·0	2·5	7·0	10·2
7 ..	10	8·2	0·6	6·0	1·9
8	10	10·1	0·5	4·1	1·5
9	6	5·5	0·5	3·0	0·5
10 ..	5	10·9	3·1	11·3	5·9
11 ..	4	7·2	2·1	5·9	4·5
12 ..	3	5·6	1·6	3·1	1·2

It will be observed that there is a considerable and consistent reduction in blink rate for each individual when following an oscillating, or relatively complex, visual stimulus compared with the rate when following a straight, or reasonably simple, stimulus, using a direct control. Using the formula

$$\sigma_{d_M} = \sqrt{\sigma_{M_1}^2 + \sigma_{M_2}^2 - 2r_{1.2}\sigma_{M_1}\sigma_{M_2}}$$

(Guildford, 1942) this difference is significant at the 1 per cent. level, t being 5·39. A similar reduction in blink rate is seen between following a straight and an oscillating track using a velocity control. In this case, however, the difference is a smaller one, and is not as consistent. In one case (subject No. 1) blink rate under these two conditions is the same, while subject No. 6 shows an increase when following an oscillating stimulus. The overall difference in blink rate under these two conditions using a velocity control is statistically significant at the 5 per cent. level, $t = 2{\cdot}42$, in spite of these cases. It will be seen, furthermore, that there is a reduction in blink rate when following a straight line using a velocity control compared with that following the same stimulus using a direct control. On the other hand, the blink rate when following the two tracks is slightly higher when a velocity control is used, as compared with that when using a direct control. The variation in blink rate with the different conditions is shown graphically in Figure 2, where the mean blinks per minute for all subjects is plotted for each condition.

Figure 3A shows the successive minute by minute variation in mean blink rate for all subjects using direct control, and Figure 3B using velocity control. It will be observed that, using both methods of control, there is a regular variation in blink rate with the difficulty of the task, a relatively high mean blink rate occurring when a straight track is being followed and a relatively low one when following an oscillating track. When using a velocity control, the differences are less marked than when using a direct control, but are consistent and in the same direction.

(c) *Relation between blink rate and movement and error scores.*

Comparison of Tables I and II and Figure 3 indicate that the mean blink rate varies in a way which is inversely related to the variations in both error and movement scores. To simplify the comparison, all three variables have been plotted in Figures

4A and B, A showing the relationship when direct control is used and B using velocity control. To reduce the variables to the same range, all individual scores have been standardised by dividing each individual's score per minute by his average score throughout the trial. Scores when direct and velocity controls were used have been kept separate as stated above. Hence, the standard blink score for subject one in

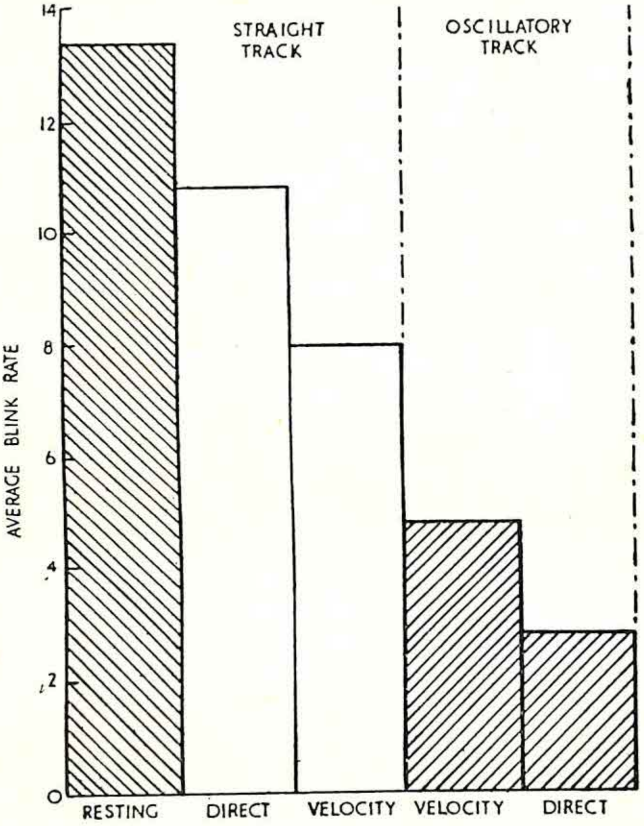

FIGURE 2

AVERAGE BLINKS PER MINUTE UNDER DIFFERENT CONDITIONS

minute one using direct control is obtained by dividing his actual blink rate during that minute by his average blink rate for all minutes using direct control, and similarly for all subsequent minutes. The score therefore represents the ratio of his actual score on any given minute to his average score for a given type of control. A score of 1·00, therefore, indicates an actual score which is identical with the average, a score smaller than 1·00 indicates an actual score smaller than average, and greater than 1·00 an actual score larger than average.

It will be observed that there is a consistent and inverse relationship between blink rate and both movement and error scores. An increase in either error or movement scores is accompanied by a decrease in blink rate and vice versa. This inverse variation is true of all individual subjects, with the exception of subject six when using a velocity control, the inversion of whose blink rate with this type of control has already been noted.

It is not possible from this experiment to define the precise relationship between blink rate and error or movement scores. It is only possible to say that the relationship is an inverse one. The experiment, as designed, gave periods of little or no response from the subject during which there was a high blink rate, and periods of relatively intense activity, during which there was a very reduced blink rate. The points obtained, therefore, define the two extremes of a curve representing the relationship between these variables. A more graded series of situations, giving graded error and response scores, is needed to enable the intervening points to be plotted.

It is also impossible to decide whether error scores, representing the difficulty of the task, or the movement scores, representing the response load, is the more important variable in determining blink rate. Those situations in this experiment which predisposed to large error scores also demanded large movements. As will be seen from Figure 4, the scores vary in the two conditions in a very similar manner.

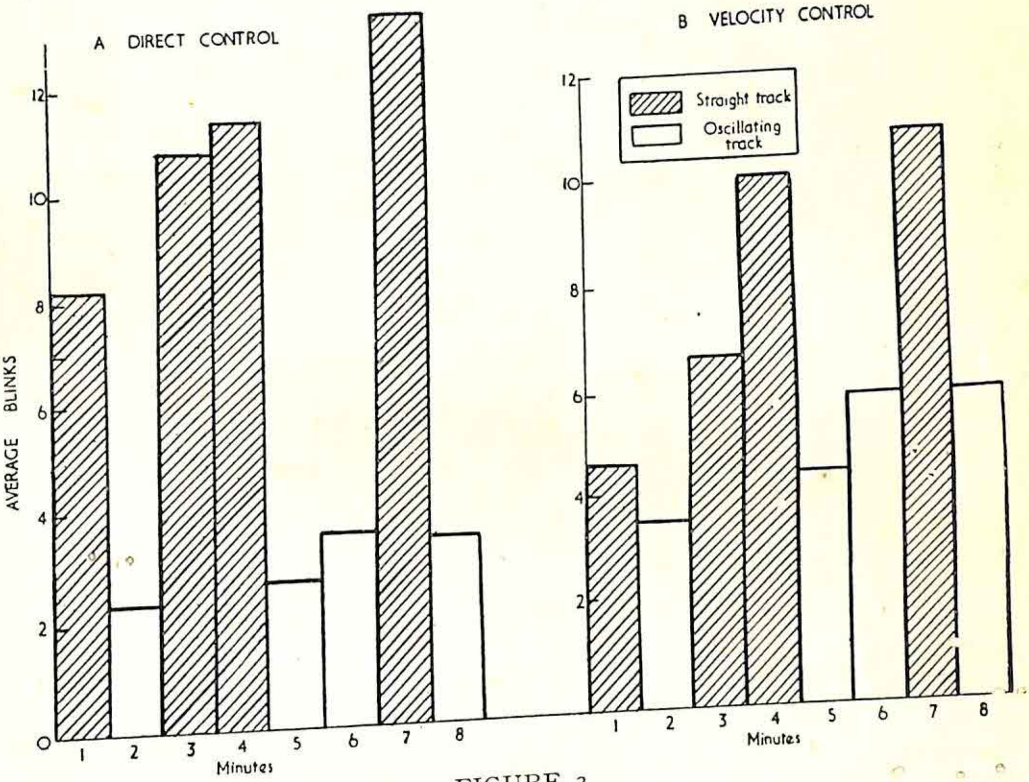

FIGURE 3

Distribution of Blinks per Minute

It will be observed from Tables I and II that there are signs of a progressive reduction in error and movement scores during successive minutes, under the same type of stimulus. This reduction in errors and movements has its counterpart in an increase during successive minutes in blinking. It seems likely that the reduction in errors and movements represents a learning curve. In any case, the increase in blink rate on the same task, as the task becomes easier, shown by the comparison of these graphs seems additional confirmation of the relationship existing between blink rate and difficulty.

An analysis of the distribution of blinking relative to the part of the track being followed suggests that the timing of blinking is also governed by the nature of the task. Such an analysis is shown graphically in Figure 5. A schematic track has been drawn, representing the straight portions on the one hand and the oscillating portions on the other. The distribution of blinks has been plotted in terms of the total number of blinks for all subjects occurring at respective portions of the track. Trials when direct and when velocity control were used have been plotted separately. It will be observed that there is a characteristic difference in the type of curve obtained when following a straight track from that when following an oscillating track. This is true with both types of control. When following the straight track with a direct control there is a steady increase in blinking from the beginning to the middle of the period, with a progressive decrease as the oscillating track approaches. With a velocity control, the picture is similar except that the build-up of the blinks is slower. When following an oscillating track, however, blinking tends to be concentrated into two main periods, just before and just after a major change of direction of the track.

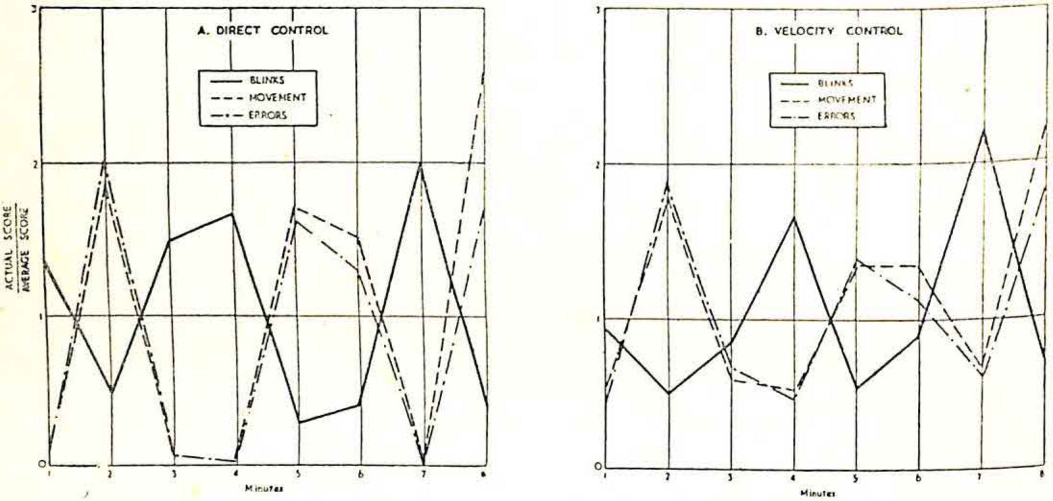

FIGURE 4

COMPARISON OF BLINK RATE WITH ERROR AND MOVEMENT SCORES

These relatively sharp bends in the track are periods of major difficulty and of major activity for the subjects. Throughout the periods of steering the pencil round these bends, of which there were 46 in each trial, there was no blinking at all. When following the more gently sloping parts of the oscillating track blinking took place at a low and fairly steady rate.

Although all subjects show a variation in blink rate with the visual task attempted, individual differences in blink rate are maintained. As will be seen from Table III, subjects with a high "resting" blink rate tend to blink at a higher rate than those with a lower "resting" rate, even during those periods when the overall rate of blinking is much reduced. Product-moment correlations of "resting" blink rate with the blink rate under each of the four experimental conditions range from + 0·68 to + 0·77. These, for this number of subjects, are all significant at the 1 per cent. level.

The results reported above demonstrate an inverse relationship between blink rate and the difficulty of the task as represented by the number of errors made. As the task increases in difficulty, the rate of blinking for each subject decreases. There is no evidence in these results, however, to support Lawson's hypothesis that the relative blink rates of particular individuals would be reflected in the relative accuracy with which they perform a visual-motor task. Correlations of "resting" blink rates with the errors made under each condition range from − 0·014 to + 0·012. When the blink rates and error scores of the various subjects are correlated under a given condition (i.e. following an oscillating track with a velocity control, etc.), the figures obtained range from + 0·064 to + 0·29. None of these figures approach significance for this number of subjects. The situation appears to be therefore, that, though individuals do differ considerably in blink rate, and the relative order of blink frequency tends to be maintained in spite of marked changes in actual blink rate, associated with the task attempted, there is no relationship between relative blink rate and the accuracy with which such a task as this is performed.

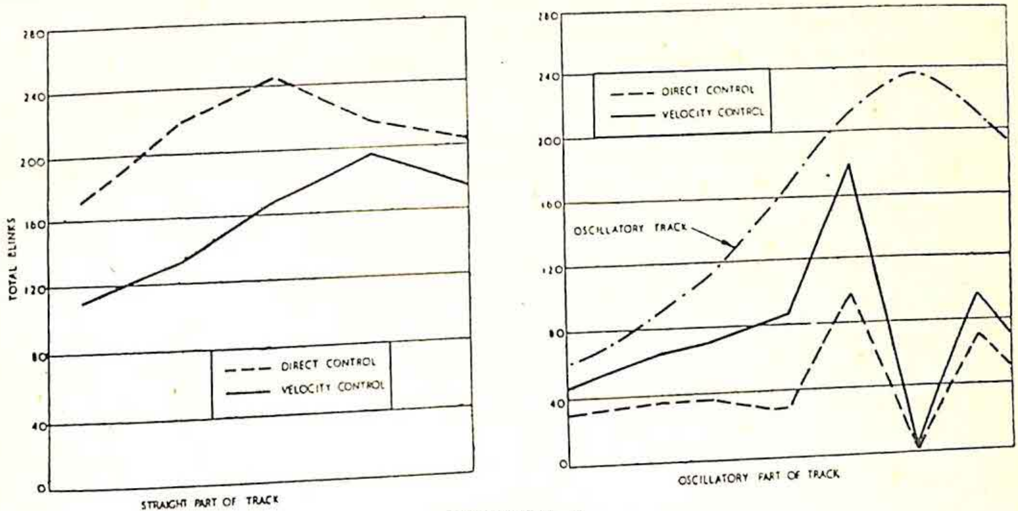

FIGURE 5

Distribution of Blinks at Different Parts of Tracks

IV

B. Car Experiment

As a check on the validity of the laboratory results in a field situation, a cine camera was fitted to a car in such a position that photographs could be taken of the driver's eyes while the car was in motion. Drivers were then instructed to drive along a previously arranged route. This route entailed driving at first in open country, then through a town where very heavy traffic conditions normally exist, and finally into open country once more. Films of the driver's eyes were taken during the three parts of the route.

TABLE IV

Subject	*Average blinks per minute*	
	Driving in heavy traffic	*Driving in open country*
1	0·9	10·2
2	1·3	8·8
3	1·2	7·6
4	0·7	6·8

The results obtained are highly significant even though adequate film records were available for only four subjects. Records for all other subjects had to be discarded since head movements for these subjects were so large as to produce periods when their eyes were not visible on the film. As head movements, and consequent gaps in the records were much greater when driving in heavy traffic, so that gaps were not randomly distributed, these subjects had to be discarded. Results obtained with the four remaining subjects are tabulated in Table IV.

It will be seen that these results strikingly confirm the results of the laboratory experiment. Blinking is reduced as the stimulus pressure and the difficulty of the task increases.

V

Discussion

The results of this experiment confirm the wide range of individual differences in blink rate noted by previous investigators, and suggest that these relative differences tend to be maintained through changes of conditions which produce major alterations in the actual blink rate. The relative order of frequency of blinking in different individuals tends to be maintained in spite of marked changes in the actual frequency. The evidence strongly suggests, however, that there is no relationship between the frequency of blinking, either during a "resting" phase, or during the actual performance of an activity, and the efficiency with which the individual is able to perform a visual-motor task such as that used.

On the other hand, it appears quite clear that when the activity is such that efficient performance depends upon efficient vision, interference from blinking is reduced to a minimum. This seems to be effected in two ways. The frequency of blinking seems to be adjusted to the demands made by the visual stimulus. Blinking is at a maximum when the visual stimulus does not demand a response, either by itself remaining constant, as in following a straight line with a direct control, or when the changes are such as to have no response significance, as in the "resting" phase of this experiment. The blink rate is then progressively reduced as the stimulus "load," and the difficulty of making an adequate response, increase. It is, unfortunately, impossible, from these results, to decide whether it is the difficulty caused by the complexity of the response, or by the speed at which responses must be made, or both, which control the blink rate, since, in these experiments both varied together. It seems unlikely, however, that the rate of change in the visual stimulus alone is the critical variable, since different blink rates resulted from using direct and velocity control mechanisms with identically changing visual stimuli. This critical variable is clearly to be found in the relationship between the stimulus and the response it demands.

The second mode of adjustment seems to be in the distribution of blinking. In a non-visual situation, such as the "resting" phase of this experiment, and in a visual task making a more or less constant demand, blinks appear to be relatively evenly distributed in time. Ponder & Kennedy and Duke-Elder & Lawson all comment on the relative constancy of the inter-blink period in their observations. When the stimulus-response relationship is a constantly changing one, however, as in the "oscillating track" portion of this experiment, the distribution of blinks changes. Under these conditions, the blinking which does take place, appears to be concentrated around but not during the periods of maximum difficulty, occurring just before and just after these most demanding parts of the task. While the exact timing of blinking is not possible in this experiment owing to the intervening reaction-time of the observer, it appears that there is some anticipatory function in the distribution of blinking. Blinking takes place at those periods when the least foreseeable interference will occur in following the visual stimulus. Hall's observations, that, when reading aloud, blinking is concentrated at punctuation marks, and especially at the breaks when turning pages, appears to support such a view. Evidence derived from the car experiment, though no more than suggestive because of the few cases involved, suggests that when the stimulus "pressure" is heavy, blinks which do occur, take place during head movements, which presumably reduces the angular velocity of visual stimuli. This is in line with the observations made by Mowrer (1933). It appears possible, from these results, too, that under such conditions the duration of the blink is reduced.

The fact that the blink rate is so sensitive to the task attempted, changing markedly from minute to minute with the task "pressure," may be responsible for some of the discrepancies in previous results. With the commonly used techniques of 5 minute sampling of blink rates at various stages of the experiment, great care would have to be exercised to control the demands made by the task if such sampling periods are to be comparable. There appears, however to be no reason to suppose that the efficiency with which a visual-motor task is performed, is in any way connected with individual blink-rates.

The writer wishes to express his thanks to Mr. B. Waldron and Miss E. Evans of the Department of Psychology, University of Bristol for their help in the laboratory experiment, and to Mr. R. Moore and his staff in the Road Research Laboratory for obtaining the cine records in the car experiment.

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MISCELLANEA

DIFFERENCE LIMINA FOR QUALITY OF PURE TONES: AN EXPLORATORY STUDY

BY

P. DENES and F. A. CARTIER

(From the Department of Phonetics, University College, London.)

It is recognized almost universally that the physical properties of sound waves, their frequency, intensity, duration and overtone structure, have a certain correlation with the perceived characteristics of sounds, pitch, loudness, length, and quality respectively. But these correlations are far from perfect and are intricately interconnected. Few researchers still think of pitch simply as the perception of the frequency of a tone, or of loudness simply as the perception of intensity. Certainly the sensation of pitch is largely determined by the frequency of a sound, but a change of intensity can change the perceived pitch of a tone; the loudness of a tone can be altered without varying its physically measurable intensity. Similarly, we know that quality, once thought of simply as perception of harmonic complexity or overtone structure, is dependent as well on intensity, frequency (Fletcher, 1935) and phase (Chapin, 1934).

It is by their differences of quality that we distinguish between, for example, a clarinet and a trumpet when both are sounding notes of the same pitch and loudness. And it is to their obvious differences in acoustic spectra that we ascribe this quality difference. But it has long been observed that pure tones have easily discernible quality differences, particularly when the difference between their frequencies is great, and, subjectively, these quality differences seem much like those by which we are enabled to distinguish between one musical instrument and another. It is clear then that that aspect of sounds termed "quality" involves more than overtone structure, and is a discernible aspect of sounds with no overtone structure at all.

Some writers have dealt with this difficulty by reserving the term "quality" for the perception of overtone structure, and have said that pure tones have different "timbres" (e.g. Fletcher, *loc. cit.*). Others have attempted to break down even this concept of timbre into various other discernible aspects and claim that timbre is, at least in part, a combined perception of the "volume," "density," and "brightness" characteristics of tones. There seems to be some evidence to justify such a breakdown, but the experimental investigations have given conflicting results (Stevens & Davis, 1938).

It has variously been suggested that the quality or timbre of pure tones may be the result of subjective harmonics, or that the mind may "supply" quality out of memory of everyday sounds. The former suggestion is amplified by the idea that low-pitched sounds seems fuller and richer because many subjective harmonics of low tones would be in the auditory range, while those for high pitches would be too high to be audible.

Hoping to clarify the situation somewhat, the experimenter (F. C.) spent the early weeks of the study simply listening to pure tones, comparing them, and making purely subjective observations which were recorded without regard for logic or theory. Attempts were made to describe and subjectively appraise even the most elusive and vague impressions. Except where noted, all observations were made at equal loudness—always at 40 phons.

It was immediately obvious that it was not only possible to observe quality differences between pure tones, but also to describe these differences in words. The descriptions were always metaphorical and never altogether satisfactory, savouring more of poetry than science, but appeared to be fairly stable; i.e. they seemed equally apt even after considerable practice in listening and after long rest periods over the course of a few months.

Some typical subjective descriptions follow:—

2,000 c.p.s. sounds "sharp and hard" when compared with the "soft, blunt, more comfortable'' 125 c.p.s.

> 500 c.p.s. sounds "purer, clearer, more bell-like" than 125 c.p.s.
>
> 250 c.p.s. sounds "somewhat narrower, thinner and not so fuzzy" as 125 c.p.s.
>
> 62 c.p.s. sounds "more rumbly" than 125 c.p.s. It seems possible to become aware of the vibrations *per second* of a 62 c.p.s. tone. There is much in common, however, between the qualities of 62 and 125 c.p.s.
>
> 1,500 c.p.s. has a more "reedy" sound than 1,000 c.p.s. 1,000 c.p.s. does not give any impression of "reedy-ness."
>
> 125 c.p.s. does not seem to have any "bell-like" quality. When compared with 225 c.p.s., the latter has a distinct "ringing, bell-like quality" which differentiates it sharply.
>
> The quality of 31 c.p.s. is rather unusual. But the intensity must be very high if it is to be compared at equal loudness with, e.g. 40 phons of 500 c.p.s. This frequency was therefore not investigated further.

It will be noted that some of the verbal "descriptions" are metaphors which might fall into the classes of "density" and "volume" as explored by Rich, Stevens and others; but other "dimensions" of quality are as readily observable and the range of metaphors which seem applicable to quality sensations appears to be almost unlimited. "Sharpness," "fuzzyness," "sourness," "richness" and other metaphors, once suggested, seem to be just as real aspects of quality as "density" and "volume." It is relevant that some people describe quality differences among sounds with chromatic metaphor, and even ascribe certain colours to certain pitches. The experimenter found it impossible to imagine colour metaphors for quality or pitch, but found it entirely possible, though difficult at first, to imagine taste-oriented metaphors. Once decided upon, the metaphors seemed strikingly apt. It may be supposed that careful experiments might yield difference limina for such "dimensions" and it may well be suspected that such limina as these would be measurements of the validity of the metaphor rather than of the sensation phenomena as normally defined.

When subjectively comparing tones of equal pitch but different frequencies (e.g. 1,000 c.p.s. at 30 d.b. sensation level has the same pitch for the experimenter as 1,010 c.p.s. at 50 d.b. sensation level) there seems to be a definite quality difference (besides the loudness difference). This quality difference does not seem to be like the quality difference between two tones of equal loudness and different frequency.

It is difficult to overestimate the factor of focus of attention on the subjective impressions received when listening to sounds. The experimenter seemed to be able to disregard almost any aspect(s) of a sound and concentrate solely on any other. After much practice at this, however, it became difficult to listen to the totality of a sound. There was instead constant shifting of attention from one aspect to another. It was found to be particularly difficult to listen to the total quality of sounds made up of two pure tones unless they were octaves or were very close together in the spectrum. It appeared that two simultaneous tones could be perceived as separate sounds under certain conditions. Indeed, by concentrating on one of the two tones, the other could be reduced to apparent non-existence.

Out of these and other observations, a problem suggested itself for experimental investigation. The problem was stated thus: Is it possible to determine frequency differential threshholds (difference limina) for the quality of pure tones?

The equipment for the experiment (which was the same as that used during the preceding observations) consisted of two sine-wave generators with maximum harmonic content of 0·25 per cent. or less, and an electronic switching device which, on pressing a button, automatically presented one tone for two seconds, then a silence of about a quarter of a second, and the second tone for two seconds. The sounds were presented through headphones. The maximum harmonic content of the acoustic output of the phones was 0·5 per cent. and was usually much less. There was no audible switching click or surge. Separate attenuators permitted adjustments of intensity so that the loudness of the two tones could be kept constant at 40 phons regardless of frequency differences (Denes & Naunton, 1950). The switching and timing device was a refinement of the device described more fully by the same authors.

The experimenter was the sole subject. All observations were made monaurally with an earphone.

The method was to set the first tone of the comparing device at a base frequency (e.g. 125 c.p.s.) with a loudness of 40 phons, and vary the frequency of the second, keeping its loudness constant at 40 phons. Thus 125 c.p.s. was compared with 135 c.p.s, then with

500 c.p.s., then with 200 c.p.s., and so on. For each comparison, a record was made of the frequency of the tones and whether or not a difference of quality was observed. Choice of frequencies for the second tone were more or less random until the general area of the differential threshold became apparent. Then frequencies were selected with an eye to narrowing the area. It was originally planned to use an equal loudness contour chart and read off the necessary attenuator settings for keeping the loudness constant at 40 phons. Such a chart was made before the experiment began. It was discovered, however, that the experimenter's thresholds and even the shape of the equal loudness contour varied quite noticeably from day to day. It was therefore found to be more convenient and accurate to balance the loudness of a small range of tones to be compared before each session rather than rely on even the most carefully made chart.

The following figures are presented only as an indication of the type of finding looked for in this preliminary work and as some proof of the plausibility of such research. Figures are averages of several trials, but represent both casual as well as careful observations.

Base frequency	*Absolute D.L. in c.p.s. frequency increase*		*Relative D.L. in per cent. frequency increase*	
125	Between 40	and 100	Between 31	and 80
500	150	280	30	56
1,000	200	350	20	35
3,000	400	1,000	13	33
4,000	600	1,000	15	25

The second column of the table may be read, e.g. "Comparing with 125 c.p.s., frequencies up to 125 + 40 = 165 c.p.s. showed no difference in quality, but frequencies above 125 + 100 = 225 c.p.s. were observed to be distinctly different in quality. Observations between 165 and 225 c.p.s. were inconsistent."

Summary and Conclusion

A single subject listening monaurally and at constant loudness of 40 phons, compared pure tones of different frequencies for differences in quality. Subjective observations indicated that pure tones of different pitches are also characterized by differences in quality which seem to be much the same sort as those by which we are enabled to distinguish between the sounds of different orchestral instruments.

It was then attempted to determine the minimum frequency-changes which would be accompanied by changes in quality, i.e. to determine the frequency differential thresholds for the quality of pure tones. It was found possible to determine such thresholds, and rough preliminary figures were obtained.

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A CASE OF ABSOLUTE PITCH

BY

A. CARPENTER

(*Medical Research Council, Applied Psychology Research Unit, Cambridge*)

People who claim to be able to place the pitch of any given musical note, without reference to some external standard but purely "by ear," seem to be regarded by many non-musicians as either misguided or dishonest. Yet musicians are quite familiar with the phenomenon, and recognize that a very reliable and accurate "sense" of this nature occurs in a small minority of their number, who are described as having "absolute pitch." This ability is certainly rare, however, and having recently had the opportunity to examine a man who claimed to possess it, and found his claims to be largely justified, I considered the results of my brief examination to be of sufficient interest to publish.

The subject was a research student in zoology and a keen amateur pianist and singer. He comes of a musical family and has been in contact with practical music all his life. The question arose because his research had included frequency measurement, and he had used his unusual ability to name the note he heard, and had then converted the musical labels into frequencies by reference to tables. I was asked, as it were, to "calibrate the apparatus" which he had used; and this I did as follows:—

Twenty-eight forks were collected, covering a range of 65 to 1,300 c.p.s., which, coming from various sources, did not form a coherent series, but were scattered irregularly, yet without major gaps, over the range. The subject, provided with pencil and paper, and his conversion tables, sat at a desk. Behind him the forks were arranged on a table from which the experimenter picked them at random, and sounded them by striking with a rubber mallet. In almost every case one blow was sufficient for the subject immediately to speak, and to write, the name of the note. Each fork was presented twice, separated by a variable number of others. In addition, the note from an electrical oscillator was sounded on two occasions, but in this case the frequency was increased from 602 to 608 c.p.s. between the two presentations. The second note was judged distinctly sharp of the first, but the difference could not be estimated quantitatively, and the two were recorded as the same note. There were thus 58 presentations, which occupied about half an hour. The subject claimed that there was no interference between each note and those which had preceded it, and his claim is supported by the finding that there was neither increase nor decrease in error as the series progressed. The forks and oscillator were calibrated after the subject had been tested, by reference to a crystal-controlled oscillator of undoubted accuracy, using an oscilloscope.

The results in summary are as follows:

(*a*) The mean error of the whole series, expressed in each case as a fraction of the known fork frequency, was 0·025. Since a tempered semitone has a frequency ratio of 1·0595, the mean error was less than half a semitone.

(*b*) Taking into account the sign of each error, the mean was plus 0·015, i.e. an average over-estimate of 1·5 per cent.

(*c*) On the average there was no difference between pairs of presentations of the forks (means: 0·026 and 0·025).

(*d*) The subject did not always agree with himself, however, about the same fork; the average difference between the pairs of estimates being 0·018.

(*e*) The electrical oscillator, adjustable over the very small range of 602–608 c.p.s., was used with the hope of exploring the least nameable difference in pitch, at that frequency. As has been said, this difference was immediately recognized, but could not be quantitatively estimated. The least namable difference appeared to be of the order of a quarter of a semitone. There were two pairs of forks (1) of 255 and 260 c.p.s., and (2) of 998 and 1,000 c.p.s., which were estimated as the same note on all four occasions in each case (1) 262 c.p.s. (c_1) and (2) 988 (b_2). When the forks of the second pair were presented in immediate succession, however, without the possibility of beats arising, that of higher frequency was immediately picked out as sharp of the other, the difference being 0·2 per cent.

(*f*) This brief experiment cannot claim to have explored the whole audible range, but as far as it went, there was no indication of appreciable change in ability with frequency. The error was approximately constant, the largest error, of 0·096, occurring at 425 c.p.s., and adjacent to some of the smallest.

(*g*) Finally, very rough tests with a beat frequency oscillator indicated that this ability extended in frequency to about 4–6,000 c.p.s. Intervals of increasing size could be recognized up to about 10 kc.p.s.

Of particular interest was the way in which the subject gave his estimates. Whereas the name of each note, e.g. "G sharp," was given at once, the octave came after a very brief interval, and the frequency was then obtained from the tables. Such doubt or hesitation as occurred was never about the name or the place in the octave, but rather about in which octave the note occurred. In fact, no octave errors were made. Bachem (1937; 1948) divides the perception of pitch, in such subjects as the present one, into (*a*) the "chroma"—i.e. the place of a note in the octave—as immediately presented to consciousness as is, for example, the colour "red"—and (*b*) the "tone-height," an approximate but quite independent impression, which gives the octave. The present rather scanty results are in agreement with this hypothesis—incomprehensible though it is to one, such as myself, who lacks the ability of "absolute pitch."

In summary, notes between 60 and 1,200 c.p.s. were immediately named with a mean error of less than half a semitone, the frequencies being given with a mean error of 2·5 per cent. Frequency differences of less than this magnitude could be recognized, but not estimated.

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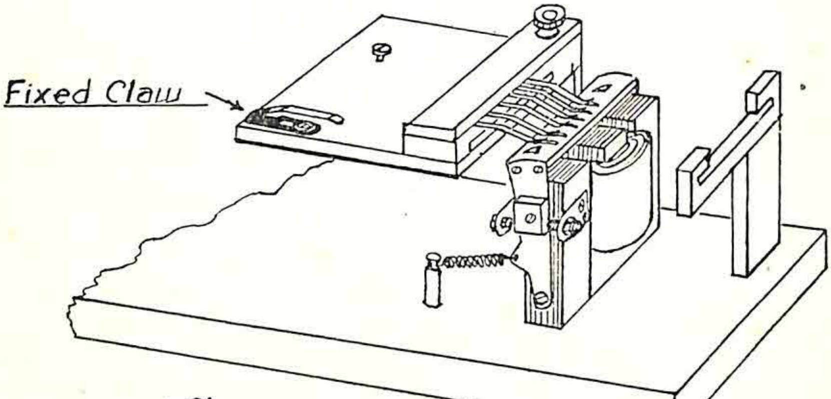
A SIMPLE STIMULUS GENERATOR

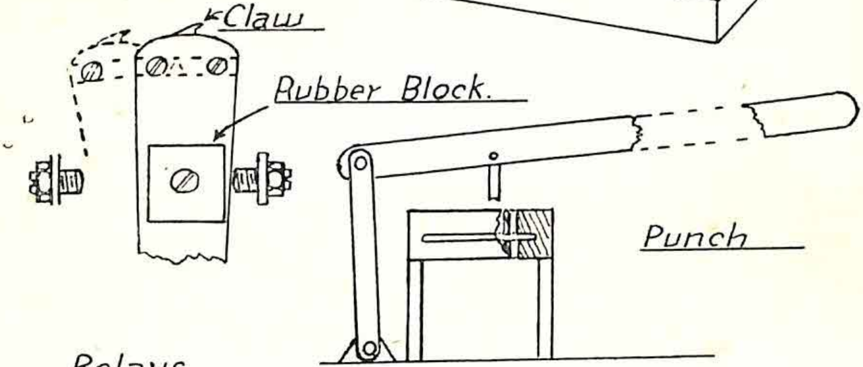
BY

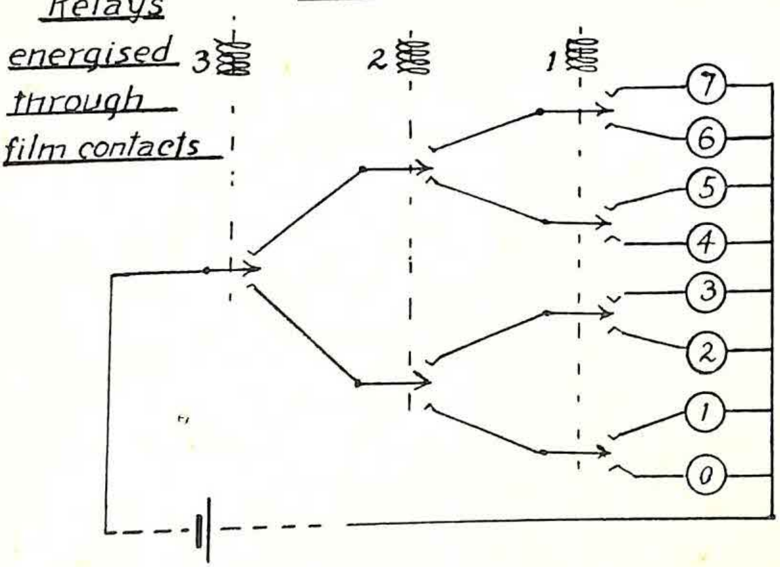
W. E. HICK

(From the Medical Research Council Applied Psychology Research Unit, Cambridge)

It is often useful to be able to present a prepared sequence of events, such as stimuli, automatically. The device described is easy and cheap to make, simple to use, and has proved very reliable.







The stimuli are represented by holes punched in 35 mm. film or recording paper. Almost any other film or tape with driving perforations could be used, but this is a convenient size, and provides a use for old stock. Claws on the armature of an electro-magnet move the film on one perforation at a time, and contacts are "made" through the punched holes. The diagram indicates the general features, but the following practical points are worth noting.

The electro-magnet is of a type obtainable at present very cheaply from dealers in surplus equipment. But any transformer or choke of similar size could easily be adapted by cutting the stampings and, if necessary, rewinding. A power of about 5 watts is required for reliable action, and the coil may have to be rewound to suit the supply voltage. D.C. is preferable, but A.C. may, of course, be used if the rather loud hum is not objectionable.

The upper contacts are strips of copper foil, 0·004 in. thick, with dimples punched in the contacting ends. The lower contacts, in the present model, are blobs of solder on strips of copper foil fixed with cellulose cement on an insulating layer on the top of the armature. The important point here is that the contact is a sliding one, which not only eliminates the necessity for non-oxidizable contact materials, but also prevents failure due to dirt between the contacts. It can be arranged that contact is made either when the magnet is energized or when it is not.

The claws must be shaped with some care, to ensure smooth engagement and disengagement. Since they slide under the film on the return journey, they should not project upwards more than necessary. A fixed spring claw (only one is required) prevents the film slipping back.

No spool is necessary for the film. The roll is simply slipped on to a rod projecting from an upright member. The sudden drag of the claws is taken up by a spring tensioner, and the apparatus works well with as much as 100 ft. of film. As there are about 65 perforations per foot, no such length is ever likely to be needed. For practical lengths, of the order of 10 ft., the tensioner could be omitted. For such lengths, also, it is far more convenient to let the film run on to the floor than to provide a take-up spool.

The punch for preparing the film happens to be separate from the present model, but could with advantage be incorporated in the device. The punch and die should be hardened and tempered, and the punch should have the usual cylindrical concavity in the end. The diameter of the holes is $\frac{1}{8}$ in., which allows six to be placed across the width of the film.

Finally, concerning the use of the apparatus, six contacts enables six circuits to be made or broken independently, and therefore six different stimuli may be given in any desired sequence, without further complication. On the other hand, by making use of combinations of holes, instead of only one hole at a time, a sequence containing up to 63 alternatives may be constructed (omitting the case of "no hole"). Although this is a larger number than will commonly be needed, it sometimes is convenient to employ the same principle for smaller numbers; e.g. up to three holes will give seven alternatives, and they can be directly recorded in the same code by three pens. But if it is necessary to transform this coded information into a selection of one out of 7 separate circuits—if 7 is the number of alternatives—a decoding mechanism must be added. The principle of a simple type, using relays, is shown in the lower part of the diagram.

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BOOK REVIEW

The Mathematical Theory of Communication. By Claude Shannon and Warren Weaver.
University of Illinois Press, 1949. Pp. 117.

Although this is not a psychological work, the current interest among psychologists and other biologists in *Information Theory* justifies some reference to it here. The publishers describe the book as "largely intended for social scientists who are concerned with . . . the broad communications problem, and for students of language," but this is stretching a point, doubtless in the hope of appealing to the wider market uncovered by Wiener's Cybernetics. In fact, however, Dr. Shannon, whose contribution is by far the larger, contents himself with a concise and austere statement of the mathematics of communication, as previously published by him in the *Bell System Technical Journal*. The theory, as he presents it, is therefore conditioned by the engineering problems, to which, as he says, the semantic aspects of communications are irrelevant. Thus, he represents the total system schematically as consisting of an information source, a transmitter, a channel, a noise source, a receiver and a destination. The source generates messages which are coded by the transmitter into signals and despatched along the channel. On their way, they are subject to more or less alteration by "noise"—i.e. by unwanted information characterized only by its statistical structure. Indeed, all the information is regarded statistically. The important entity is not the particular message, but the ensemble of messages. The information content of an ensemble, whether of useful or of "noise" messages, is an entropy, viz:—

$$H = - \Sigma p. \, log. \, p$$

where the p's are the probabilities of the individual messages. Working in terms of entropies is a convenience, whereby, for example, the necessary conditions for the complete reconstruction of the noise-contaminated message can be neatly exhibited.

The engineering bias is obvious from the foregoing remarks. It must be emphasized, however, that Dr. Shannon is dealing with the philosophy of the process; nowhere does he refer to specific "hardware." Indeed, his most concrete examples are concerned with language. For instance, he shows that the redundancy of English permits an almost grammatical sentence to be constructed purely from the statistical nature of the language; and that the redundancy is just small enough for large crossword puzzles to be possible, but not for three-dimensional crossword puzzles. These, of course, are merely illustrative asides. The real meat is in the mathematical concepts and technique, which range from the easy to the difficult, providing something for everybody. Much of the difficult matter could have been made relatively easy by fuller explanation, however.

It is a pity also that no mention is made of the theory of structural and metrical information developed by Gabor and MacKay, or of Fisher's statistical information. All these are logically related, but the general scientific reader still awaits a comprehensive treatise on the fundamental notion of information.

Dr. Weaver's contribution is to explain, in plain English, what Dr. Shannon is talking about, and to suggest broader interpretations; both of which tasks he discharges admirably. In particular, he makes it clear that the exclusion of the semantic aspect is a result of the engineering application rather than an inherent deficiency in the theory. Information without meaning always strikes the newcomer as rather meaningless, if one may put it so; it is therefore good to have this reminder that the separation is not absolute.

In short, this little book is an excellent introduction to the subject, as much for those who merely want an impression of its scope and nature as for those who are interested in studying it seriously.

W. E. H.

EXPERIMENTAL PSYCHOLOGY GROUP

MEMBERS

Babington Smith, B., O.B.E., M.A. (Hon. Treasurer).	Institute of Experimental Psychology, 34, Banbury Road, Oxford.
Carpenter, A., M.B., B.Chir.	Psychological Laboratory, Downing Place, Cambridge.
Chambers, E. G., M.A.	Psychological Laboratory, Downing Place, Cambridge.
Crawford, Miss A., M.A., Ph.D.	Department of Psychology, The University, Liverpool.
Crown, S., M.A.	Institute of Psychiatry, The Maudsley Hospital, Denmark Hill, London, S.E.5.
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Drew, G. C., M.A., B.Litt. (President)	Department of Philosophy and Psychology, The University, Bristol.
Eysenck, H. J., M.A., Ph.D.	Institute of Psychiatry, The Maudsley Hospital, Denmark Hill, London, S.E.5.
Foss, B. M., M.A.	Institute of Experimental Psychology, 34, Banbury Road, Oxford.
Grindley, G. C., M.A., B.Sc.	Psychological Laboratory, Downing Place, Cambridge.
Harris, A. D., M.R.C.S., L.R.C.P. (Committee)	Psychological Laboratory, Downing Place, Cambridge.
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Poulton, C., M.B., B.Chir.	Psychological Laboratory, Downing Place, Cambridge.
Russell, Prof. R. W., M.A., Ph.D.	Psychology Department, University College, Gower Street, London, W.1.
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Trist, E., O.B.E., M.A.	Institute of Human Relations, 2, Beaumont Street, London, W.1.
Vernon, Miss M. D., M.A. (Committee)	Department of Psychology, The University, Reading, Berks.
Vince, Miss M. A., M.A.	Psychological Laboratory, Downing Place, Cambridge.
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Whitfield, J. W., M.A. (Committee)	Psychology Department, University College, Gower Street, London, W.1.
Zangwill, O. L., M.A. (Hon. Secretary)	Institute of Experimental Psychology, 34, Banbury Road, Oxford.

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TRANSFER OF TRAINING AND SKILL ASSUMPTIONS IN TRACKING TASKS

BY

C. B. GIBBS

(*From the Medical Research Council Applied Psychology Unit, Cambridge*)

Matched groups of subjects were used to test the learning and transfer effects that follow changes in the display, the muscular reactions and the directional relationship between stimulus and response in a tracking task. Two arrangements were compared in the relationship studies: one arrangement of the stimuli and reactions was similar, and the other was opposed to that used in many every-day skills. The familiar arrangement was easier to learn. There was high positive transfer from the unfamiliar to the familiar, and little transfer from the familiar to the unfamiliar.

The physical dimensions of the display were varied to give two tasks with different stimuli. The initial learning times were equal for both tasks, and the transfer between them was high, positive, and equal. Two further tasks varied in the extent, speed and force of the required muscular movements. One task proved more difficult to learn initially, and there was greater transfer from the difficult to the easy task than from the easy to the difficult. A further experiment tested the effects of changing the difficulty of a tracking course, and it was found that learning was more rapid on the more difficult course. A difference in difficulty between two tasks, therefore, determined both the amount of transfer between them and the rate of learning the tasks.

New measures were developed to test the transfer between tasks of unequal content, and the effect of such inequalities upon the rate of learning. The findings are discussed, as are their possible implications for transfer measurement and their bearing upon existing theories of transfer.

I

INTRODUCTION

THE theory of identical elements appears to dominate the analysis of the problems of transfer, together with the view that transfer depends, in some way, upon the similarity of the stimuli and responses in the first and second task, as measured along some physical dimension. This theory is no longer adequate by itself, but it summarizes some major trends in the experimental data. It has been demonstrated, for example, that in some tasks the physical characteristics of the stimuli and muscular reactions may be altered over a certain range, but these differences do not alter the nature of the tasks so far as learning and transfer are concerned; and the subject can readily transfer from any task within this range to any other task that also lies within this range. Bartlett (1947) has pointed to similar zones of equivalence in performance tests, and has developed the concept of *limits of ability* to define these zones. Here the physical variables of a task may be altered over a certain range without affecting the standard of performance. It is possible that there is a close relationship between the zones of equal performance and those of equal transfer, and the problems of transfer measurement and prediction would be greatly simplified if such a relationship could be demonstrated.

Such a connection may well exist in certain tasks which vary only in the physical dimensions of the stimuli and reactions, but these are not the only elements that make up the total content of a task, and it is here that the second concept is introduced. The stimuli and reactions may be related in a way that is expected or unexpected in relation to the experience of the subject before the experiment, and this factor may dominate the initial learning and the transfer between two tasks. This is a most important aspect of learning and transfer research which has so far been greatly neglected, apart from the preliminary work of Vince (1944), Warwick (1947) and Mitchell (1947) which has been summarized by Mitchell and Vince (1950). From previous experience there are built up a large number of *skill assumptions*; the subject may be quite unaware of the existence of them although they are used many times in the day. He will expect that, if he moves a control lever to the right, then some element in the visual display will respond by moving to the right rather than to the left. This simple assumption is clearly based on long experience dating from infancy. But, starting much later in life, it is possible to build up extremely extensive and complex skill assumptions as, for example, any car driver finds he has adopted, when he changes from a country with left-hand to one with right-hand rule of the road. Then, again, much shorter term skill assumptions are found when a subject begins to expect from his recent immediate experience of a changing display, that the signals will subsequently follow the initial patterns they presented in space and time (Poulton, 1950). A similar static instance occurs when one expects to find stationary objects where they were last seen.

The real importance of skill assumptions is found in transfer experiments when one of the tasks is in conflict with everyday experience. The expected task will undoubtedly be easier to learn, whether it is given before or after the unexpected task. In the extreme case, for example, even if there were no learning during the initial unexpected task of a transfer experiment, there might well be apparently complete "transfer" towards a task organized in a way that everyday experience had taught long before the subject had entered the laboratory.

Two procedures were used in these investigations:

1. The handle-winding task.
2. The steering task.

II

HANDLE-WINDING EXPERIMENTS

Procedure.

Apparatus. The task given was that of compensatory tracking, and control was by handwheel. An electric motor tended to rotate a pointer at a constant rate away from a datum position. The pointer tip was visible to the subject, and his handwheel control was connected to the pointer through a mechanical device, that added or subtracted the handwheel drive and that of the electrical motor.

The result was that when the subject turned the handwheel continuously and smoothly at rates of 18 turns per minute (i.e. at the same rate as the electric motor, but in the opposite direction) the two drives cancelled out, and the pointer tip remained stationary against the index mark.

This was the operator's task, and any pointer movements were therefore errors. These degrees of rotational error were summed on a further dial and recorded every minute.

Practice periods and criterion. The subjects practised winding continuously for five-minute periods with a three-minute rest period between each practice. Only three such periods of practice were given in any one forenoon or afternoon. The criterion of complete learning adopted and used for all groups was that errors should not exceed 5° over the whole five-minute period. Scores at this level were then examined in detail to determine learning times to the nearest minute, since a score could be less than 5° over a five-minute period, but the score for any one minute might exceed 1°. This could occur at any time between the first and fifth minutes, and the learning scores were summed to the minute when such errors last occurred. In effect, the 5° criterion determined learning times to the nearest five minutes, and the 1° standard defined the nearest minute. After attaining this level of ability on the first task, all subjects and all groups continued practice on their initial task, to a total of 40 minutes' practice, to equate practice for all groups before transfer.

They then transferred to their second task, and the time to reach the criteria on this task was recorded, again to the nearest minute.

Variations of the apparatus. The speed of the electric motor remained constant in revolutions per minute throughout all the tests, but nevertheless a change in the apparatus that substituted a different *length of pointer*, resulted in an entirely different velocity of the pointer tip, for any given error in the winding rate of the handwheel. The stimuli were therefore altered. The corrective movements of the handwheel, that were associated with particular errors were also altered, i.e. the ratio of hand to pointer movements, or gear ratio, was also varied by this change.

Changing the *handwheel size* altered the basic speed and distance moved by the hand in maintaining a constant rate of winding. The ratio of hand to pointer movements (gear ratio) was also altered. The hand moved through different distances and at different speeds, to bring about any given correction at the pointer. A further change in the physical conditions also resulted, because a different basic force was needed to rotate the handwheel, when the handwheel diameter was changed.

The *direction of winding* could also be easily reversed. One arrangement needed a constant rate of *clockwise winding*, but, by changing the direction of rotation of the electrical motor, *counter-clockwise* winding was required from the subject.

The *sense of the directional relationship* between handwheel and pointer could also be varied. Clockwise handle-winding could result in clockwise pointer movements, or, by using a reversing gear in the drive between handwheel and pointer, in counter-clockwise movements of the pointer. (Fourteen subjects were used in a preliminary experiment to determine the preferred or expected relationship. Their responses showed that 12 subjects expected that a clockwise handwheel movement would result in clockwise pointer movement.)

Five out of all possible arrangements of these variables were selected for study:

1. *Standard arrangement.*—The standard arrangement of the apparatus; this was:
   (i) Small handwheel of 3-inch diameter, requiring a basis force of 0·75 lb.;
   (ii) Small pointer of 3-inch radius;
   (iii) Clockwise direction for handle-winding;
   (iv) Expected directional relationship; clockwise rotation of hand giving clockwise pointer movement.

2. *Unexpected arrangement.*—This changed only one factor in the standard arrangement—the directional relationship; clockwise movement of the hand gave a counter-clockwise movement of the pointer.

3. *Large pointer arrangement.*—Also differed from the standard arrangement in only *one* respect, the large 12-inch pointer was used instead of the small 3-inch pointer. This quadrupled the speed error of the pointer tip that resulted from any given error in winding rate and altered the gear ratio by 4 : 1.

4. *Counter-clockwise arrangement.*—Again this altered one factor in the standard arrangement; counter-clockwise turning was required instead of clockwise.

5. *Large wheel arrangement.*—Was also different in one respect from the standard arrangement; the large 6-inch diameter wheel was used instead of the small 3-inch wheel. The extent and speed of hand movement was doubled, and the basic winding force was halved. The gear ratio was altered in the ratio of 2 : 1.

The matching and allocation of the groups.

Altogether 70 experimental subjects were tested, all naval ratings aged between 19–25 years. By giving everyone a preliminary practice spell of one minute on the standard arrangement (1) of the equipment it was possible to divide the 70 subjects into seven groups of ten men each (Groups A, B, C, D, E, F and G) and to ensure that the average initial ability of any one of these groups was the same as that of any other.

Two of these groups were used to compare the effects of any one factor; one group learned the standard, followed by the modified arrangement; with a second group this order was reversed.

III

RESULTS OF HANDLE-WINDING EXPERIMENTS

The average learning times for each group of subjects were used to compare the effects of the different experimental arrangements. The data have been summarized in Table I.

TABLE I

AVERAGE TIME (IN MINUTES) TO LEARN THE FIRST AND SECOND TASKS

Nature of problem	*Group*	*Initially learned arrangement*	*Initial learning time*	*Transfer to arrangement*	*Transfer learning time*	*Total time to learn both arrangements*
Change of directional relationship	A	Expected	20·5	Unexpected	25·7	46·2
	B	Unexpected	30·0	Expected	1·9	31·9
Change of pointer size	C	Small	18·3	Large	2·7	21·0
	D	Large	20·0	Small	2·6	22·6
Change of direction of winding	E	Clockwise	19·0	Counter-clockwise	2·3	21·3
Change of handwheel size	F	Small	19·6	Large	1·0	20·6
	G	Large	11·1	Small	3·7	14·8

Change of directional relationship.

The results in Group A and Group B showed that the change from the unexpected to the expected arrangement was much more readily undertaken than was the change from the expected to the unexpected. The preliminary practice of Group A on the expected arrangement did not reduce the time required to learn the unexpected arrangement (25·7 minutes) by a significant amount compared with the average time for Goup B (30·0 minutes), who learned the unexpected arrangement first. ($t = 1{\cdot}05$—not significant. A value for t of 2·101 was required in all these experiments to show a significant difference at the $P = 0{\cdot}05$ level.)

On the other hand, Group B learned the expected arrangement very rapidly after their previous practice on the unexpected arrangement. This time (1·9 minutes) was compared with that of Group A (20·5 minutes), who learned the expected arrangement first ($t = 7{\cdot}52$, indicating a high degree of significance). Learning the unexpected arrangement first therefore saved 18·6 minutes in learning the expected arrangement.

The total time to learn both the unexpected and the expected arrangements was markedly different in the two orders; practice on the unexpected relationship first (Group B) meant that only 32 minutes were required to learn both tasks, whereas 46 minutes were needed by Group A ($t = 2{\cdot}86$).

The initial learning times showed that the unexpected arrangement was definitely more difficult since Group B took 30 minutes, instead of the 20 minutes required by Group A for the expected arrangement ($t = 2{\cdot}54$).

Change of pointer size.

The results in Groups C and D showed that altering the gear ratio, and hence the visual stimulus in magnitude or speed, gave a high positive transfer. Transfer was substantially the same whether it was in the direction large pointer to small pointer, or in the reverse order, and total times to learn the two tasks were not affected by the sequence in which they were learned, since these times were 21·0 and 22·6 minutes respectively. In both groups the amount of skill transferred was large, positive and highly significant. A comparison of the time required by Group C to learn after transfer (2·7 minutes) with the time taken by Group D (20 minutes), who learned the same form of the task as an initial task, gave $t = 6{\cdot}96$ for transfer from small to large stimulus. Similarly, a t value of 5·56 was found for transfer from large pointer to small, the average times here being 2·6 minutes to learn after transfer, and 18·3 minutes with no preliminary training. There was no significant difference in the initial learning times of the two tasks. It was evident therefore that changes in visual stimulus and gear ratio did not affect learning times, rate of learning, or amount of skill transferred.

Change of direction of winding.

The results in Group E showed good transfer between two arrangements where response was altered from clockwise to counter-clockwise turning; only 2·3 minutes were required to learn the second task after 19 minutes had been spent in learning the first task.

Change of handwheel size.

The initial learning times of Groups F and G showed that there was a significant difference in the times required for the initial learning of the two tasks differing in the size of handwheel ($t = 2{\cdot}85$). The small handwheel took 19·6 minutes to learn, and the large took only 11·1 minutes. This difference in difficulty between the two tasks was not associated with any reliable advantage in presenting one task before the other, in order to reduce the total time to learn the two tasks; the difference between the two sequences of presentation gave a t of 1·67, which was not significant at the $P = 0{\cdot}05$ level.

On the other hand, there was greater transfer from the small handwheel to the large than in the opposite order of learning, since the large handwheel only required an average of one minute to learn, after the preliminary practice on the small handwheel; whereas 3·7 minutes were required to learn the small handwheel when preliminary practice had been on the large. This was a significant difference, t being 3·7. Learning either arrangement first resulted in high positive saving on the other as a second task, and the benefits of the preliminary training were highly significant in both cases ($t = 6{\cdot}3$ and $t = 5{\cdot}0$).

IV

Steering Task

Procedure.

The task of serial pursuit was set in this experiment, and the subjects tracked along a moving line of electrical contact studs with a pointer carrying a single ball contact. This pointer was moved by a handwheel, and the task was like steering along a winding road. One course was simple to follow, with few curves, whereas the other was difficult, with frequent and sharp curves. It was relatively easy to anticipate correctly in one task and also easier to memorize the course. In both courses the subjects could only see two contact studs in advance of the pointer, and the rest of the course was hidden. Visual anticipation time was constant in both courses, but one course was more like that expected by most people in continuous tasks, i.e. that the signals remain relatively constant, and there is a good deal of repetition (C. Poulton, 1950). One group had all their initial practice of 21 runs on the difficult course and were then transferred to the easy course. This Group (H) was made up of 10 business women of ages 32–52. The other Group (I) learned the same tasks but in the opposite sequence. They were 10 naval ratings of ages 19–25 years, and they were considerably better than the women at a short preliminary test of the same kind of skill. It was intended to see whether a good system of training could make a poor group more accurate than a good group provided with only an indifferent training.

The method of scoring was that contacts, or "hits," were only made when the subject kept the pointer exactly in line with the moving line of contact studs. These hits were added by counter to give the score for any one run. The highest possible score on either course was 194 hits. Two points in each learning curve were taken to compare the groups on the steering tests. These were the group score on their 21st and final run on the first task, and their first score on the second task. These scores were directly compared, and they were also added to compare the overall ability of the two groups at the same stages in their training.

V

RESULTS OF STEERING EXPERIMENT

The group average scores are set out in Table II.

TABLE 2

RESULTS OF STEERING EXPERIMENTS

Group	*Final score on first task*	*Initial score on second task*	*Sum*
H: Business women—difficult task first easy task second	165	182	347
I: Naval ratings—easy task first difficult task second	181	144	325

In spite of their low initial ability the training of Group H on the difficult task resulted in a more rapid extension of their abilities, and at the end of their training they were superior to Group I, on both the task that they had practised and the final task that was new to them. Group H averaged 347 hits for the difficult plus the easy run, whereas Group I averaged only 325 hits; this difference was highly significant, t being 3·5. It is of particular interest that the first score of Group H on transfer to the easy task was the same as the final score of Group I on the easy task, although the latter had practised this task *directly* for 21 runs, with all the advantages of memorizing that this implies. Indirect was therefore equivalent to direct practice. This is an interesting finding, and when the innate inferiority of Group H and the greater memorizing advantages of the younger Group I are taken into account, it seems likely that indirect practice was possibly superior to direct, although this has not yet been proved. It follows, however, that a carefully adjusted training can greatly accelerate learning rates, even in a poor group, and show a very real extension of general ability, as compared with a superior group who were not exercised at full capacity.

Transfer effects were high and positive in both cases. But initial training on the easy task gave much less transfer to the difficult task, and the inferiority of direct to indirect practice in this case was highly significant ($t = 4·3$). Group H on the difficult task scored 165 hits after 21 runs of direct practice, and Group I scored only 144 hits in their initial run on the difficult task, after their preliminary training on the easy task.

The apparent difference in difficulty was confirmed, since Group H increased their score from 165 to 182 on transferring from the difficult to easy track ($t = 6{\cdot}7$), and it has been shown that practice on the difficult task was equal to practice on the easy task. Also Group I scores decreased from 181 hits on the easy task to only 144 hits on the difficult task ($t = 8{\cdot}5$).

VI

Discussion

Test design for handle-winding experiments.

A new test design is needed to study the effects upon transfer of skill assumptions or unequal task content, since the standard designs use equal tasks, or pool the data when unequal tasks are used, and are not suited to the experimental purpose. The conventional transfer formulæ cannot be used to show the transfer interactions between unequal tasks, because the percentages are determined by two factors: the amount of skill carried over from the first to the second task, and the content of the second task. If two tasks are unequal, and are presented in opposite order, then the calculated transfer percentages must also tend to be unequal; but it cannot be assumed from this that the transfer of skill is different in the two orders of presentation since two effects are confounded in the percentages formulæ. Thus the measures of the average learning time for each task, and the total learning time for each pair of tasks, have been used in the handle-winding experiments. The former measure can be used to show the time saved in learning each second task because of the preliminary training on the first. The levels of task difficulty, and the amounts of skill transferred, can therefore be related to each other. The latter measure shows the relationship of task difficulty and the order of presentation, to the total learning time for both tasks.

The total learning time is an essential measure. It is evident that time will be lost in learning the more difficult task first, but there may be a compensating saving in learning the easy task after transfer. There may be an equal, but opposite effect in the reverse order of learning; so that the total learning time is the same and unaffected by the order. This is defined as simple additive compensation.

Other tasks may show a difference in total learning times, depending upon the learning orders, and such tasks must be identified and treated separately.

Test design for steering experiments.

The steering test did not proceed to complete learning of either task. The test was designed to compare the effects of task content upon the *rate of learning* two tasks based upon a common skill, rather than the absolute amount of transfer that might have been found if learning had been complete.

Simple relationship between task content and amount of skill transferred.

The handwheel-size and pointer-size experiments (Table I) show that there was more transfer from the difficult to the easy task than from the easy to the difficult, and equal transfer between the tasks when they were of equal difficulty. The unequal transfer of the handwheel-size experiment cannot be explained by theories which state that the amount of transfer is related to the degree of similarity of the stimuli and responses along some physical dimension. It is clear that the separation of the physical factors is the same no matter in which order two tasks are presented, and transfer would be equal if it depended only on physical separation. A new approach has to be made to the problems of generalization and discrimination in transfer.

The stimuli and reponses were changed substantially in the experiments on pointer and handwheel size, but there was high positive transfer between all such variations. It follows that when one arrangement of the physical variables was learned, this learning extended over a wide range of these variables. The pointer-size experiment gives the simplest case, with equal learning times, and very high transfer in both directions between the two arrangements, in spite of considerable alterations in the stimuli. The two arrangements must indeed have been virtually the same task, from a psychological point of view, in spite of large physical differences.

The concept of different levels of required ability.

This indicates that a concept of *levels of required ability* may be important in transfer. Bartlett (1947) has outlined the concept of limits of ability. He pointed out that wide variations can be made in physical factors without any corresponding change in human performance, provided these changes lie within a certain range on the physical scale of the environmental factors in question. "The fundamental features of a performance will remain stable over a certain range of its conditions. Outside this range they will change often in a dramatic and radical manner." A major purpose of performance and learning tests is to determine the range over which a physical condition can be varied without affecting performance or learning scores. When the physical variables are altered, the tolerance limits can be defined, beyond which the task will show an increase in difficulty. Tasks within an optimal zone are psychologically equivalent in difficulty and in the level of ability they require; a common measure of such zones is that a given proportional change in the stimuli or responses is psychologically equivalent, e.g. the Weber or Fullerton Cattell Square Root Law applies. The results of the present transfer experiments suggest that tasks within such limits may not only be equal in performance and in required ability, but also in transfer, i.e. there is high and equal transfer between any two tasks within these limits. This finding deserves extensive study to discover how general it is.

Tasks outside the optimal zone require superior ability to reach a given standard of performance than tasks within the zone. It is clear that the level of ability gained in practising a task must be limited by the content of that task; thus the ability to deal with difficult situations cannot be acquired in practising easy ones. The present results could be explained therefore by a hierarchy of levels of required ability, with the higher levels including the lower. Practising a task to total possible skill may develop a level of ability that is directly related to the difficulty of the task, and there would then be high positive transfer from difficult to easy tasks and less transfer in the opposite direction. The higher levels of ability could then deal with a larger range of physical variables in the stimuli and responses than could the lower.

Relationship between measures of performance and of transfer.

The connection between learning and performance tests on the one hand and transfer tests on the other has been emphasized, since if such a connection exists then there is little point in carrying out laborious transfer tests on certain classes of physical variables, e.g. when it is known that a range of such variables conform to Weber's Law. There would also be important implications for applied work if the data from some types of performance tests could be used in the practical problems of transfer in the field.

It is clear that, when there is simple additive compensation in transfer between any two tasks, there is a direct relationship between such transfer tests and performance and learning tests that use the same experimental material. This must follow because the first part of a transfer test (the initial learning or performance test on two tasks) is in itself a performance and learning test. If there is simple additive compensation, the difference between the scores on the second tasks is equal to the difference between the scores on the first tasks, and a simple relationship must therefore exist between such transfer results and performance and learning tests. It may be important therefore to isolate the class of effects that lead to simple additive compensation in transfer, and to compare the present transfer results with independent learning and performance tests that have used the same experimental variables.

The studies of changed pointer size show equal times to learn the large or small pointer arrangement, and equal transfer from small to large, and from large to small. The change of pointer size altered the speed of the pointer tip and the gear ratio. Hick (1950) has shown that in judging stimulus speed there is an optimal range of speeds that conforms to Weber's Law. Vince (1946) has shown that tracking with a continuously changing gear ratio gives a performance score that is equal to a constant ratio.

There is also the evidence from the changes in handwheel size. The large handwheel was easier to learn as an initial task, but there was less transfer from large to small. The effects were approximately compensatory, however, since there was not a significant difference in the total time to learn the two tasks in the different orders of presentation. The basic physical force was lower, and the tangential speed of the hand was higher when the large handwheel was used, and these were nearer to the optimal conditions that have been defined in independent performance tests, *cf.* Hick (1945) and Helson (1949). These two different tasks had different levels of difficulty, as shown in the initial test, and transfer was found to be approximately proportional to the difference in difficulty.

It is evident that the results of the above transfer tests could have been predicted, with fair accuracy, from the independent performance tests. These tests altered only the physical dimensions of the basic stimulus and response, and the tasks required the same kind of skill. It is important to note that learning approximated to total possible skill in all tasks before the groups were transferred. These are essential conditions for the hypotheses that follow.

The results of the pointer size studies suggest the hypothesis (i) that *transfer between two tasks, both of which lie between the same tolerance limits, as defined by performance tests, can be* (a) *positive,* (b) *large, and* (c) *equal and unaffected by the order of task presentation.*

The handwheel results suggest the hypothesis (ii) that transfer between two tasks, one of which lies within the tolerance limits, and one of which lies outside this optimal zone can be (a) *positive,* (a) *large,* (c) *unequal and affected by the order of task presentation; this implies greater transfer when the first task lies outside the optimal zone, and the second lies within it, than when the first task comes within and the second outside.*

The changes in directional relationship between handwheel and display.

The change in directional relationship gave results that accord with hypothesis (ii), since the transfer from the difficult and unexpected relationship to the expected was very much greater than the transfer from the easy to the difficult task. This result also supports the concept of a hierarchy of levels of required ability rather than that of physical similarities, but there are several reasons why this study must be treated separately.

It is important to note that the basic stimuli and responses were the same in both the unexpected and expected versions of this task, i.e. they were common elements in both tasks. Nevertheless, the transfer from the expected to the unexpected organization of these elements was very small and had no significant effect in reducing the time to learn the latter task. This suggests that the elements of stimuli and responses may have comparatively little effect in transfer, whereas their relationship in space and time may be all important. Indeed, Bartlett (1947) has previously made the same point that stimuli and muscular reactions by themselves are not meaningful elements in the analysis of given tasks, whereas the organization of these in space and time may well be all important. This view is supported by comparing all the transfer effects of the handle-winding experiment in Table I. The change in directional relationship had a far greater effect on transfer than any of the isolated changes in stimuli or muscular reaction.

The effect of changed time-space relationships upon transfer.

It was found in the handle-size experiment that the low level of ability that was gained in practising an easy task made some contribution to the higher level of ability that was required in a more difficult task, and this contribution was roughly proportional to the difference in the difficulty of the tasks. This was not the case in the directional-relationship studies, where learning the easy task first did not result in any significant saving in learning the difficult task after transfer, whereas learning the difficult task first gave very high transfer to the easy task. There is a simple explanation of this finding, since if the essential organization of the expected arrangement had already been learned and skill assumptions built up before the experiments began, then there would be the appearance of positive transfer to it as a second task, irrespective of the relevance of the learning that took place during the course of the experiment.

Conversely, if the unexpected organization was not learned in everyday experience and was not present in the expected arrangement (that was learned as a first task by Group A during the experiment), then no knowledge of this unfamiliar arrangement could have been carried over from the expected to the unexpected arrangement or from previous experience. The learning of the unexpected arrangement had to start virtually from scratch, in spite of the preliminary training on the expected arrangement.

The skill assumption that was present in the expected directional relationship, and carried over from everyday experience, was that the subjects expected that any given reaction would move some element in the display in the same sense as the response. This assumption is clearly developed in the co-ordinations that are used in writing, pointing at a moving object, and in other everyday skills. Many other skill assumptions are to be found in serial motor skills, and several examples have been given in the Introduction. Their effect is apparently to form a preference for certain forms of organization of the stimuli and responses in space and time. It seems unlikely that simple additive compensation will be found when skill assumptions prove to be correct in one task, but not in the other. The effect is that transfer scores cannot be easily predicted from performance data in these cases, although it is conceivable that such prediction can be made when the physical dimensions of the stimuli and responses are the only elements that are altered.

It may be generally true, however, that, if new stimuli and responses are learned in a familiar relationship, this learning will make little contribution toward learning a subsequent task, where the same stimuli and responses are organized in direct conflict with the familiar relationship. There may indeed be negative transfer in some cases, since the initial learning might reinforce skill assumptions that have to be discarded in the second task.

The differences in total learning times.

The results show that in all cases it was better to teach the more difficult task first to obtain the greatest *amount* of transfer between the tasks.

This is not always the case if the total learning time or *rate of learning* both tasks is taken as the criterion. In the handwheel-size experiments there was not a shorter total learning time when the difficult, small handwheel was presented first, in spite of the greater transfer from the difficult to easy task.

The directional relationship experiments with the handle-winding task and the steering experiment showed nevertheless a higher overall rate of learning when the more difficult task was presented first. This is shown by the superiority of total scores with this order. It is clear that the rate of learning is affected by the content of the initial task, but an increase in task difficulty does not automatically increase the rate of learning. It is more probable that there is an optimal level of task difficulty, and an optimal rate of presenting new items to replace those already learned, for every kind of learning material.

The transfer bias between unequal tasks.

The differential effect of unequal task content upon transfer has been known for a considerable time; hence the directions in the early transfer tests that equal tasks should be used, or that data from unequal tasks should be pooled. It appears that Cook (1937) first realized that the effects of task difficulty merited separate investigation, but it is only recently that interest has been revived in the problem, and this has led to a number of simultaneous and independent investigations—e.g. Szafran and Welford (1950), Gagne *et al.* (1950), Gibbs and Clutton-Baker (1950) and several other British and American psychologists who have not yet published their findings. There is a considerable amount of evidence from these sources, and it is generally found that there is more transfer from the difficult to the easy version of a task than in the opposite order. It is unfortunate that these independent findings cannot yet be directly compared because the effect can arise from different causes. The present test design appears to give a method of analysing the general effect into its separate causes, i.e. by avoiding the confusion arising from the use of transfer percentages and by distinguishing between measures of time saved and of total learning time.

VII

CONCLUSIONS

1. A design that estimates the transfer between unequal tasks should use measures that distinguish between the amounts of transfer and the rates of learning, e.g. between time saved and total learning time. Transfer should not be expressed in percentages or proportions.

2. It appears that the amount of transfer between two equally difficult tasks may be equal, whereas the transfer between two unequal tasks may be unequal. There may be greater transfer from the difficult to easy task than from the easy to difficult, if the same kind of ability is required in both tasks and learning is carried on until total possible skill is closely approached in both tasks.

3. The effect may be explained by the concept of a hierarchy of levels of required ability; it cannot be explained by the theory that transfer depends only upon the separation of stimuli or reactions along some physical dimension. The detailed causes of the effect may lie in the differences in experimental learning, or in the previous experience of the subjects.

4. A change in the space/time relationship between a stimulus and response may have a greater effect on learning and transfer than a change in the physical dimensions of the stimulus or required response. Learning new stimuli and responses in a familiar relationship may make no contribution to the subsequent learning of these same stimuli and responses in an unfamiliar relationship.

5. It may be generally true that if tasks are of unequal difficulty because of a difference in the physical values of the stimulus or response, then the differences in transfer may have a simple arithmetical relationship to the differences in task difficulty. In these cases, the transfer effects may be predicted, at least approximately, from the data of performance and learning tests that have used the same experimental material.

6. But this simple relationship between task difficulty and the amount of skill transferred is not likely to be found if previous experience aids performance on one task and hinders it on the other.

7. The level of task difficulty affects the rate of learning (i.e. total learning time), but it cannot be held that an increase in task difficulty will inevitably result in an increased rate of learning. It seems more likely that there is an optimum range of task difficulty that ensures the most rapid learning.

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CHANGES WITH AGE AND WITH EXCLUSION OF VISION IN PERFORMANCE AT AN AIMING TASK

BY

J. SZAFRAN

(From the Nuffield Research Unit into Problems of Ageing, Psychological Laboratory, Cambridge)

Forty industrial workers between 20 and 60 years of age took part in the experiment. Each sat facing a display in an enclosure. His task was to locate targets with a pointer, under two conditons: when he was able to make whatever use he chose of direct vision, and wearing a pair of red goggles through which only the diplay could be seen.

On the average, those over 40 years of age took more time than younger subjects over those portions of the task which were concerned with the initiation of a fresh action. When unable to make use of vision the older had far greater difficulty in locating the targets and attempted to supplement tactile and kinæsthetic cues by making postural adjustments, turning their heads and often their bodies in the direction of the target at which they were aiming. These findings are discussed in relation to an earlier observation of the greater tendency for older people to look at what they are doing.

I

INTRODUCTION

Two previous studies by the writer (reported by Welford, 1951) suggested that younger subjects tended to carry out a part of a simple perceptuo-motor task without the aid of visual cues more often than older subjects. Although this occurred during a relatively unimportant phase of an action, there were reasons to believe that as a result the nature of the whole performance was affected. It has further been demonstrated (Welford, 1951) that in studies of ageing measures of overall performance tend to obscure important differences in the constituent reactions of a skill.

Accordingly, the attempt has been made to consider further the importance of visual cues for older people and to study the constituents within a relatively simple skill. Starting with a hypothesis that younger people are more likely than older to be able to carry out accurate limb movements without looking where they are reaching, an experiment was designed to investigate the efficiency with which people of different ages can locate by hand various positions near them in space, with and without the aid of vision.

II

METHOD

The subject sat in the centre of a wooden framework and faced a display of 19 small electric lights. He was surrounded by a corresponding number of 14-inch square targets, each carrying a round 1-inch diameter metal plate as bull's eye. These targets were arranged in various directions, but at a constant distance, within easy reach, of about 22 inches as measured from the midpoint between his shoulders. Considered vertically, six targets were at the subject's shoulder level, seven 45° above and six 45° below. Considered horizontally, one target was directly in front of the subject and 45° above his shoulder level, three targets 45° to the right side and three 45° to the left, three targets 90° to the right and three 90° to the left, three targets 135° to the right and three 135° to the left. The space directly in front of the subject at 45° below his shoulder level, and also all the space 180° behind him were not used. The space directly in front of the subject and at the level of his shoulders carried the panel of 12 v.6 w. bulbs which were arranged in such a way as to represent a plan of the layout of targets. Half-way between the subject's seat and the display was a small pillar carrying a 4-inch square metal plate mounted so as to be at the level of the subject's knees.

The subject was instructed to hold a metal stylus ¼ inch in diameter and 6 inches long in a vertical position on the plate in front of him and upon noticing a light on the display being switched off to move the stylus to the centre of the corresponding target, contact with which caused the absent light to reappear on the display. When this was achieved, he was required to return to the starting point.

The centres of the targets, the bulbs which served as target indicators on the display, the plate in front of the subject and the stylus held by the subject were wired to pens recording on a band of paper moving at a constant speed over a drum. These pens registered the following information about each complete operation:—

Time for the initiation of response. The time elapsing between the presentation of the signal and the initiation of the response to it (i.e. lifting the stylus from the plate).

Outward-transit time. The time taken to move the stylus from the plate to make contact with the target indicated.

Search time. The time required to locate the centre after contact had been made with the target area.

Bull's eye time. The time spent in contact with the bull's eye.

Return time. The time taken to bring the stylus back to the starting point.

The apparatus was essentially like that employed by Fitts (1947). However, whereas in Fitts's arrangement the subject's performance was measured in terms of accuracy of his initial contact with each target irrespective of the time taken, the apparatus used in the present experiment measured performance in terms of time taken to achieve a predetermined level of accuracy.

It was explained to the subject that he could carry out the task in any manner he liked, but that he was not allowed to touch any part of the apparatus with his hand. No mention of speed or any other criterion of efficiency was made.

After a demonstration trial every subject carried out the task under two conditions, Condition A always coming before Condition B.

Condition A: the subject was able to make whatever use he chose of direct vision.

Condition B: the subject wore a special pair of red goggles through which he could see the filaments of the bulbs forming the display, but nothing else. Subjects were not told that they would have to face this condition until after they had completed their performance under Condition A.

Every subject was given two series of ten targets in each condition, first holding the stylus in the dominant hand and then holding it in the other hand. For a right hand performance the target directly above the display and the nine targets of the right bank were given. In a left hand performance, the same target above the display and the nine targets of the left bank were employed. The order of presentation of targets was randomized in four Latin squares, allocating a different arrangement to every series in every condition. All the age groups received the same overall treatment.

It was originally intended to compare Condition A with Condition B, which would have necessitated half the subjects receiving the latter first. Preliminary trials revealed, however, that the difficulty of the task in Condition B when it came first was so great that many subjects, especially those in the higher age groups, gave up. Condition A was given first, therefore, to all subjects.

There were ten subjects in each of the four age decades from the twenties to the fifties. They came from the Trumpington Industrial Hostel, Cambridge, and were of various European nationalities.

When the distributions of all time measurements falling within each age range were inspected separately for each constituent reaction, it became obvious that the amount of overlap between the age groups was very large in each case, and that the variance tended to increase, often very markedly, with age, especially in respect of the scores for Condition B. All these distributions were of the asymmetrical type, positively skewed to various degrees and few of them could be described adequately by the arithmetic mean and standard deviation. In common with many other time scores, however, the logarithms of the scores were distributed approximately normally, but even so the variances remained different.

The *individual* distributions of time scores, as opposed to the *age range* distributions described above, displayed wide variations from subject to subject. It was difficult to see what measure other than the arithmetic mean would be more representative of the central tendency of these varied distributions. Accordingly, although both geometric and arithmetic mean values were computed for each age group, the significance of observed differences was assessed from the arithmetic mean values per subject, using the method developed by Kendall (1948) and Whitfield (1947).

III

RESULTS

In treating the results no distinction was made between the two hands since the scores for each followed the same patterning of constituent reactions studied. The separate measurements were combined, and the mean value per hand was used for the purpose of further descriptions and analysis.

Condition A

TABLE I

MEAN TIMES (IN SECONDS) PER SUBJECT AND DIFFERENCES BETWEEN AGE GROUPS. CONDITION A.

(Geometric mean values given in brackets).

Age group	*Initiation of response*	*Outward transit*	*Search*	*From first contact with "bull's eye" to beginning of return movement*	*Return*	*Total time*
Twenties ..	0·75 (0·73)	0·99 (0·96)	0	0·47 (0·44)	0·93 (0·90)	3·14
Thirties ..	0·85 (0·83)	1·11 (1·07)	0	0·73 (0·68)	0·96 (0·92)	3·65
Forties ..	1·03 (1·00)	1·07 (1·06)	0	0·57 (0·55)	0·93 (0·90)	3·60
Fifties	1·06 (1·01)	1·27 (1·08)	0	0·91 (0·88)	1·00 (0·95)	4·24
			For differences beetween age groups			
Subjects ranked by age ..	$P<0{\cdot}01$	$P>0{\cdot}1$	—	$P<0{\cdot}01$	$P>0{\cdot}1$	$P<0{\cdot}01$
Dichotomy: Twenties/Fifties:	—	$P<0{\cdot}06$	—	—	$P>0{\cdot}1$	—

The total times per target together with the times for the several constituents of the performance are summarized in Table I. Four points may be noted about these results:—

1. On the average the performance of each of the four age groups was built up in the following manner: About one quarter of the total time was spent in initiating the response, about one third in moving the stylus to the target, about one sixth at the bull's eye, and one quarter in bringing the stylus back to the starting point. It is interesting to note that the outward transit time tended to be longer than the return time, which in respect of movement was essentially its equivalent.

2. The mean overall time per target for the oldest age group was about 35 per cent. higher than for the youngest. The constituent times contributing to this

increase with age were the initiation of response and the bull's eye time. The outward transit time showed a tendency in the same direction, whilst the return time remained essentially the same for all the age groups.

3. All subjects made full use of the opportunity to see while aiming at the targets—hence the complete absence of search time scores. Subjects' introspections suggested that seeing the stylus on the bull's eye rather than the restoration of the absent light in the display served to indicate success. Moreover, the experimenter observed that, upon making contact with the bull's eye, some subjects would immediately bring the stylus back to the starting point, while others would do so only after they had taken a swift glance at the display. Unfortunately, it was not possible to make this observation in such a way as to be certain whether or not it had any relation to age.

The differences in the values of initiation of response time minus outward transit time and of outward transit time minus return time gave probabilities of 0·001 and 0·01 respectively.

Condition B

TABLE II

MEAN TIMES (IN SECONDS) PER SUBJECT AND DIFFERENCES BETWEEN AGE GROUPS. CONDITION B.

(Geometric mean values given in brackets).

Age group	*Initiation of response*	*Outward transit*	*Search*	*From first contact with "bull's-eye" to beginning of return movement*	*Return*	*Total time*
Twenties . .	0·96 (0·92)	1·36 (1·28)	16·28 (7·75)	0·97 (0·88)	1·20 (1·12)	20·77
Thirties . .	1·13 (1·01)	1·29 (1·21)	17·15 (8·95)	1·06 (0·89)	1·38 (1·28)	22·01
Forties. . . .	1·55 (1·36)	1·21 (1·08)	22·58 (11·27)	1·63 (1·30)	1·58 (1·26)	28·55
Fifties	1·67 (1·50)	1·17 (1·11)	35·96 (19·08)	2·49 (1·73)	1·60 (1·36)	42·89
	For differences between age groups					
Subjects ranked by age . .	$P < 0\cdot001$	$P > 0\cdot1$	$P < 0\cdot001$	$P < 0\cdot001$	$P < 0\cdot1$	$P < 0\cdot001$
Dichotomy: Twenties/Fifties:	—	$P > 0\cdot1$	—	—	$P < 0\cdot05$	—

The results for Condition B are set out in Table II. Comparing the results in Condition B with those in Condition A, it will be at once clear that performance in Condition B took a much longer time and showed substantially greater changes with age. The main points are:—

1. The search time scores which were absent in Condition A assumed here a preponderant place, constituting some four fifths of the total time.

2. The mean overall time per target increased substantially and consistently with age, making the oldest subjects' scores twice that of the youngest. However, the performance was differently built up in the different age groups.

3. Those in the forties required on the average about 60 per cent. more time than those in the twenties to initiate the response. Those in the fifties showed a further increase of some 10 per cent.

4. No well defined differences between the groups were detected in the outward transit time. What mean trend there was, however, was in the downward direction with age. Although not significant in itself, this trend appeared of some importance when viewed against the initiation of response time. Relative to this, the youngest age group spent some 40 per cent. *more* time in the outward transit. Those in the thirties already displayed a slight shift in the opposite direction, taking only 14 per cent. more time. However, those in the forties required for the outward transit some 20 per cent. *less* time than for the initiation of the response, and those in the fifties a further 10 per cent. less still.

It should be borne in mind that the task was such that after the signal had been presented to the subject, he could interpret it at any time he chose between the moment he perceived the signal and the moment he made the first impact with the target area. This part of the performance embraced the first two constituents, the initiation of response and the outward transit. The combined scores indicate a slight increase with age, amounting to some 20 per cent. between the youngest and the oldest group.

5. The time required to locate the centre of the target increased very substantially with age. Those in the forties spent about 40 per cent. more time than the youngest on this, and those in the fifties a further 60 per cent. The time required was thus doubled from the youngest to the oldest age group.

6. It is not meaningful to give the times spent in the bull's eye for Condition B. The difficulty arises because it often happened that subjects entered the bull's eye, but went on searching apparently unaware that the display light had been restored. The mean percentage of such occurrences is given in Table III, showing a considerable increase in the forties and fifties. In all these cases of "accidental entry," the attempt to locate the bull's eye continued until at some later stage it was realized that success had been attained. Clearly, what objectively was a bull's eye was not necessarily so subjectively. The time for this stage of the performance has, therefore, been taken as the interval that elapsed between the moment an objective change in the display took place and the moment this change was apprehended by the subject so as to cause him to withdraw the stylus from the target. Those in the forties required about 55 per cent. more time than those in the twenties to become aware of and to take action after such a change in the display had taken place. Those in the fifties required some 45 per cent. more time still.

TABLE III

Mean Percentage of Occurrences of "Unnecessary Search" per Subject

Age group	*Mean per hand*
Twenties . .	28·5 per cent.
Thirties . .	33·0 ,,
Forties . .	51·0 ,,
Fifties . .	74·5 ,,

7. As far as could be judged from the gross head movements, most subjects turned their heads and sometimes their bodies in the direction of the target at which they were aiming. They seemed to be attempting, although according to their introspective reports without being aware of it, to produce the same postural adjustments they would have made had they been able to see. These head movements were marked among subjects of all ages during the initial phase of the operation, and among the older subjects also during the greater part of the search. The younger subjects, however, tended during the search to watch the display—a behaviour upon which, it will be remembered, Condition B places a premium. The continuing of the attempt to locate the bull's eye after the light had reappeared was no doubt due to the fact that the subject's head was turned away from the display at the time.

8. The mean time required to bring the stylus back to the starting point showed a non-significant tendency to increase with age. The oldest group was some 20 per cent. slower than the youngest.

It is noteworthy that the mean return time, relative to the outward transit time was over 10 per cent. *lower* for those in the twenties, was almost equal for the thirties and was over 20 per cent. *higher* for the two older groups, whereas in Condition A it was lower for all age ranges.

9. At the end of the experiment subjects were encouraged to describe the subjective phenomena. When the goggles were removed, almost every subject spontaneously made remarks to the effect that the targets seemed to have been located further away than they actually were. The wording of these comments was usually such as to emphasize the subject's astonishment at how his senses had deceived him and how much nearer in reality he was to the targets. Their other remarks about Conditon B were not particularly helpful, for in this connection they referred as a rule to Condition A, in which, they claimed, they had "learnt the layout." Requested to estimate the total time spent in the experimental enclosure, most subjects overestimated by some 50 per cent. On the whole, they were puzzled and amused by the task.

One subject deserves separate mention, as showing what appears to have been an occupational transfer effect (Szafran and Welford, 1949). When he was submitted to the test, it was not known that he had had considerable experience of work in the dark-room of a photographic laboratory. His performance was for obvious reasons not included in the results of the main experiment. He was 49 years old, and by all standards his was the most efficient performance observed. Although showing the patterning of the initial phase usual for older subjects, he required less time to locate the bull's eye than the mean of the youngest age group. Also he took approximately as long as the mean of this group to react to the restoration of the display light. He practically never turned his head from the display.

With advancing age there is a decline in the efficiency with which small details can be visually discriminated (e.g. Weston, 1949). It was thought desirable, therefore, to ascertain whether the increase with age in the initiation of response time in Condition B was due to inability to see the lights or to something else.

Accordingly 18 subjects from 20 to 60 years of age were asked to lift the stylus from the starting point as soon as they noticed a light being switched off. They did not have to locate any target. Precautions were taken to ensure that they could react only to the visual signal and not to any noise produced by the switches. Every subject received ten signals for each hand in two conditions: not wearing the red goggles; wearing the red goggles.

It was found that the mean difference between these conditions lay between 15 per cent. for the younger and 25 per cent. for the older subjects. Since the percentage differences between the initiation of response times in Condition A and Condition B of the main experiment for the corresponding age ranges were of the order of 30 per cent. for the younger and of 60 per cent. for the older, it seems clear that the differences in the initiation of response time in the main experiment were not due solely to difficulty in seeing the display.

IV

DISCUSSION

With increase of age and transition from Condition A to Condition B there were substantial changes of performance, but they did not occur to the same extent in all the constituents of the task.

The changes with age seem to have been of three kinds.—Firstly, the older subjects tended to take longer than the younger over those portions of the task which were concerned with the *initiation of a fresh action*. Thus, in Condition A, the older tended to take longer to initiate both the outward and the return movements, although the times for the *execution* of these movements were about equal for all age groups. Likewise, in Condition B, older subjects were slower at initiating their outward movements, although the movements themselves tended to get *faster* with age. The initiation and making of the return movements in Condition B cannot properly be compared with those of Condition A, since they were likely to have been affected by the subjects' being unable to see the targets and the plate on the central pillar. The finding that older subjects took longer to initiate new responses is in line with those of a previous experiment by Brown (reported by Welford, 1951).

Secondly, the older subjects tended to be far more affected than the younger by being deprived of vision in Condition B—as is shown by their markedly longer times to locate the centres of the targets. It is possibly for the same reason that they also took longer to return from the targets to the plate on the central pillar.

Thirdly, there was a persistent tendency on the part of the older subjects to make in Condition B postural adjustments similar to those made in Condition A, when they could look at the targets while locating the bull's eyes. They were, it seemed, reinstating the postural patterns of Condition A in spite of the fact that looking away from the display made them unable to see when they had located the centre, and thus lengthened the time between hitting the bull's eye and initiating the return movement. This behaviour is interesting in view of the tendency noted in previous experiments (reported by Welford, 1951) for older subjects to look away from a display to what they are doing. Between the earlier experiments and this, there is the important difference that whereas in the former the visual information obtained *might* have been of some usefulness, in this experiment vision was excluded, and the postural adjustments must therefore have been made for some reason other than the obtaining of visual information.

What this reason is can only be conjectured at present, but two possibilities deserve consideration: First—that the postural adjustments in Condition B were patterns of action transferred from the preceding Condition A and may be regarded as due to the persistence of a habit and inability to "unlearn" a useless action—this observation has precedent in an experiment carried out by Kay (reported by Welford, 1951); second—that the proprioceptive data resulting from the postural adjustments in some way aided the older subjects in locating the centres of the targets. In other words, the older subjects required more data in order to carry out the task, and they obtained it by reinstating the postural adjustments made in Condition A.

While it is impossible definitely to choose one of these possibilities to the exclusion of the other on the data from the present experiment—it would, indeed, be very difficult in *any* experiment—such evidence as there is seems to favour the second rather than the first. For not only were the older subjects less efficient without vision, but also they were in many cases unable to carry out the task in Condition B at all unless they had done it first in Condition A, although they inspected the display and the targets before the experiment began. Thus they illustrated what Sherrington has described as "the inefficacy, as a means to arrive at a new motor correlation, of instruction merely verbal, or of ideas constructed without motor experience." Clearly something transferred from Condition A enabled the older subjects to carry out the task in Condition B, and it seems reasonable to suppose that this was a series of proprioceptive patterns.

The experiment was originally suggested by Professor Sir Frederic Bartlett and was carried out under the general supervision of Mr. A. T. Welford. Mr. J. A. Leonard designed and constructed the electrical circuit, in consultation with Mr. N. T. Welford. The writer wishes to thank Miss V. R. Cane and Mr. A. E. D. Schonfield for some helpful criticism.

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THE OPERATION OF AN ACQUIRED DRIVE IN SATIATED RATS

BY

K. DANZIGER

(From the Institute of Experimental Psychology, Oxford)

A series of four short experiments indicates that the behaviour of satiated rats in a runway, at the end of which they have previously been rewarded, differs significantly from the behaviour of satiated rats without previous reward in the experimental situation. The former group reach the end box more quickly after having been put in the starting box of the runway and if provided with food in the end box proceed to eat it, although they have just refused similar pieces of food in their home cages. This is shown not to be due to defective satiation or the operation of fear in the control group. When runs and feeding in the end box are separated during the training period, the previously rewarded group still shows a more vigorous response on satiated trials, indicating that it is a reward expectancy about the goal box rather than a running habit which has become "functionally autonomous," acting as a situation-specific drive.

INTRODUCTION

MOST experiments on animal learning make use only of the basic physiological drives because of their dependability. The result is that the animal almost never gets an opportunity to demonstrate any ability it may have to learn drives as well as perceptual and motor habits. This has led some psychologists to infer that only the basic physiological needs do in fact act as drives in sub-primates, and Allport (1947), for instance, concludes that the gap between human and animal motivation cannot be bridged.

Nevertheless, a few recent experiments have demonstrated the operation of acquired, situation-specific drives in an animal like the rat. In particular, it has been established that fear repsonses may be acquired (Estes & Skinner, 1941, May 1948, Miller, 1948b, and Mowrer, 1940); that is to say, a neutral stimulus repeatedly presented with an electric shock may eventually produce marked avoidance behaviour when presented alone. It is not possible, however, to generalize about drive acquisition from this evidence, as it is obtained from an abnormal situation highly disturbing to the animal.

Drive acquisition based on the food-seeking tendency is not nearly so well corroborated as drive acquisition based on fear. The best report is that of Young (1946) which shows that rats can acquire specific food habits which may persist in opposition to the physiological need of the animal.

The well-known work of Anderson (1941) on what he called "drive externalization" is perhaps only suggestive. Non-rewarded and satiated rats were shown to give evidence of maze learning provided they had previously been rewarded for learning another maze. But serious doubt has been cast on the efficacy of Anderson's satiation technique and on the significance of the response measure he used which may have shown up simply some transfer of training effects (Siegel, 1943).

Apart from mere restatements of observed facts the only attempt at providing a theoretical basis for the element of learning in motivation has been made by Hebb (1949). He demonstrated that even in the case of an apparently innate drive like hunger, there is a learned element when it comes to eating in a particular situation. He regards hunger as an organized neural activity with a motor facilitation of the various activities of eating and with numerous connections to other organized neural systems. From this it seems to follow that the tendency to eat may be aroused by the activity of other systems corresponding to perceptions or symbolic activities, possibly even in the absence of whatever indications of internal need normally play a part in arousing it. If this is so, it would provide a basis for specific acquired drives of a rather situation-bound type. Anderson's "externalization" principle by which a drive at first aroused by internal factors comes, by a process of learning, to be aroused by external factors suggests much the same thing.

But there are difficulties in these conceptions. So far as the acquisition of the new element is concerned, it is not clear whether the mere presence of an unsatisfied drive in a certain situation suffices or whether it is necessary for a consummatory act to occur. It is also important to know whether the new element operates like an innate drive, activating appropriate instrumental and consummatory acts. None of these questions can be decided on the basis of existing investigations.

It was therefore decided to set up a series of experiments designed to show whether and how originally neutral stimulus-situations associated with hunger and food reward were capable of affecting the behaviour of satiated rats. For this purpose it was necessary to measure performance in a very simple situation, such as a straight runway which the animal knew well, so that cognitive learning was at a minimum and did not obscure the results. It was also important to make sure that the primary drive had been effectively satiated when the animals were tested for the existence of acquired drives.

Apparatus

The apparatus consisted simply of a runway 5 feet long and 5 inches wide with sides of plywood 1 foot high and a perforated zinc floor. At either end this runway was attached to an enclosure 9 inches × 10 inches × 12 inches; one of which was painted black and used as a goal box. During the preliminary trials, while the rats were getting used to the apparatus, the tops of the two end boxes were coverd by netting. During the experimental trials rats only very rarely attempted to jump out of the apparatus and so the netting was folded back. The runway also contained three hurdles placed at regular intervals. They consisted of blocks of wood the same width as the alley, 4 inches long and 1 inch high. They were introduced to slow up the rat's running speed so that any errors in measurement would form a less appreciable fraction of the observed value. The black end box contained a shallow white dish in which could be placed a food pellet on rewarded trials.

The whole apparatus was placed on a concrete bench in the same room as the rats' home cages. It was not possible to control illumination from day to day, but if this was a disturbing factor, which seems rather unlikely, it must have affected experimental and control animals equally. Random auditory stimulation in the animal house may have produced occasional high time scores, but hardly any consistent effect on the results of the experiments, as the animals, being used to this sort of stimulation, showed quite a high tolerance for noise.

Subjects

The rats used in these experiments all belonged to the same strain of hooded animals bred originally by the National Institute for Research in Dairying at Reading. They were all three to four months old at the start of their experimental training. Some animals were rejected in the early stages of the experiments for persistent failure to adapt to experimental conditions, shown by refusal to run, squealing, jumping, defecation, etc.

Control of Feeding and Satiation

For a fortnight before the first preliminary trial the rats were all handled daily by the experimenter in order to tame them. During the last few days of this period their food in the form of dry rat-cake nuts was supplied only once a day for 90 minutes, at the end of which period it was removed. In this way a 24-hour feeding schedule was established, and this was maintained throughout the experiments. It ensured a constant and maximum degree of hunger immediately before the daily feeding period and a pretty complete state of satiation at the end of that period. Tests made during preliminary experiments had indicated that this method of satiation was as effective as more tedious and elaborate methods. Rats would always stop eating spontaneously after 60–80 minutes, so that the food was never removed when an animal was still eating. Water was available in the home cages at all times.

The food pellets used as an incentive on rewarded trials consisted of the same composite rat food that made up the rats' daily food ration. They were prepared by adding starch to a wet mash of rat-cake nuts, rolling the resulting dough, cutting it into even pellets and then drying these in an air oven. The weight of the pellets was in the neighbourhood of 0·22 gm., varying slightly, but never by more than 0.04 gm. Thus a particular rat's reward might fluctuate by almost 50 per cent. from one day to another. However, as an investigation by Miller & Lawrence (1947) seems to show that it needs at least a fourfold difference in the amount of food reward to bring about a significant change in the rat's behaviour, these variations may be neglected.

EXPERIMENT I

The object of this experiment was to determine whether a simple instrumental act, like running to a goal-box would be performed more vigorously by satiated rats with previous experience of hunger and reward in this situation than by satiated rats having the same acquaintance with the runway, but never having experienced hunger or reward in it. Such a difference in performance would have to be put down to a difference in motivation rather than in ability, and with the internal innate elements at a minimum for both groups the difference would indicate the effectiveness of learned elements in the motivation of the first group.

Procedure

After a period of preliminary handling each of the 12 male rats used in the experiment was left in the apparatus for a period of eight minutes after it had received its daily food ration. On the succeeding three days the rats were left in the apparatus for a three-minute period after receiving their daily food ration. The novelty of the situation ensured very active exploration of the runway, even though the animals were not hungry. In this way the rats were familiarized with the experimental situation and their initial fear response was greatly reduced, but there was as yet no association of either hunger or food with the experimental situation.

On the fifth day of the experiment the animals were divided into two groups of six rats each. The experimental group were given one rewarded run daily 15-30 minutes before their daily food ration was due until a total of 12 such runs had been made. On the succeeding five days they were given one run daily to an empty end box immediately after having received their daily food ration. The control group was given 17 successive daily runs to an empty goal box immediately after satiation. The crucial comparison is between the performance of the two groups on the last five trials.

The method of measuring performance at each trial was as follows. At the beginning of each trial the rat was dropped into the starting box facing the wall opposite the alley entrance, so that in order to run down the alley it had to turn about. As the rat touched the floor of the starting box the experimenter started a stopwatch equipped with two hands and stepped back behind the starting box to a position in which he was invisible to the rat but from which he could just observe the entrance to the alley. When both the rat's forelegs were actually in the alley and out of the starting box the first hand of the watch was stopped. The measure obtained was therefore one of the speed with which the rat reacted to the starting box by setting off on its run down the alley. When the rat had three feet inside the end box, the second hand of the watch was stopped so that the difference between the first and the second readings gave a measure of the time taken by the rat to traverse the 5-foot alley. On reaching the goal box after each trial the rat was placed in a small separate cage for two or three minutes before being returned to its home cage. Time readings were taken to an accuracy of 0·1 second.

Retracing was not prevented, because the retracing animal is obviously poorly motivated to reach the other end of the runway, and this should show in his time score. The occasional very high time scores produced in this way can be prevented from weighting the results too heavily by converting all scores to a logarithmic scale before comparisons are made. This is necessary for time scores in any case and is the usual procedure for dealing with this type of data. Its statistical soundness is well established (Gaddum, 1945).

It may be thought that the latency of response measure would not be reliable because of slight variations in the way the animals were dropped into the starting box, but in fact it was found that this measure was less subject to random fluctuations than the running speed.

On the first rewarded trial many of the animals in the experimental group ate the reward spontaneously, while the others could be readily coaxed to do so by offering the reward pellet by hand in the end box. On all succeeding trials consumption of the reward was immediate and spontaneous. These observations indicate that the four days of preliminary training were rather effective in reducing the initial fear response to the experimental situation.

RESULTS

Running time in the alley and response latency in the starting-box were summed for each trial to give the total response time which provided a good composite measure of performance. This was converted to logarithmic form, and the mean log total response time was calculated for each trial. During the first 12 trials the performance of the experimental group improves progressively, but on the 13th trial which is the first satiated trial, there is a sharp increase in time, though not nearly to the response level of the control group. Table I compares the performance of experimentals and controls on the last eight days of the experiment. On trials 10, 11, 7, 12, the experimentals were still running hungry and rewarded. Trials 13–17 are the test trials on which both groups are run satiated and without reward.

TABLE I

1,000 X MEAN LOG TOTAL RESPONSE TIME ON LAST EIGHT TRIALS

	Trials							
	10	11	12	13	14	15	16	17
Experimentals	653	584	581	940	657	800	888	990
Controls	1,257	1,141	1,140	1,228	1,233	1,202	1,105	1,333

In testing for the significance of the difference between groups on test trials we have to take into account that individual performances, by control animals in particular, are subject to random variation over a considerable range. This leads to very high standard error estimates which reduce the significance of the difference between trial means. We can, however, make an overall comparison of the two groups on all the five test trials, by taking the average performance of each rat for all the five test trials and then comparing the values so obtained for the two groups. In this way day-to-day fluctuations will be minimized for each animal and a much better distribution on which to do the test will be obtained.

TABLE II

1,000 X MEAN LOG TOTAL RESPONSE TIME OF ALL FIVE TEST TRIALS FOR EACH RAT

	Rats						*Overall mean*
	1	2	3	4	5	6	
Experimentals	880	776	750	851	862	1,010	855
Controls	1,296	1,496	1,434	1,290	732	1,074	1,220

It will be seen from Table II that with one exception the rats in the experimental group are all responding more quickly (i.e. make lower time scores) than those in the control group. The exception is one rat in the control group which responds faster than any other rat in any group. Notes on this animal show it to have been very active not only in the experimental situation. This type of gross individual difference does occur occasionally in work of this kind, especially if the strain of experimental animals is not highly inbred.

Mean log values for the running speed and reaction latency of each rat were calculated, giving a set of pairs of overall mean values which can be compared by the rest. (*See* Table III.)

TABLE III

Comparison of Average Values of Performance over Five Test Trials

	Total response time		*Running time*		*Reaction latency*	
	Exp.	*Contr.*	*Exp.*	*Contr.*	*Exp.*	*Contr.*
Mean ..	855	1,220	659	1,022	338	647
t ..	2·78		2·74		3·14	
P ..	0·02		0·02		0·01	

For 10 degrees of freedom the 1 per cent. level of confidence is actually reached by a *t* value of 3·17 and the 2 per cent. level by a *t* value of 2·76.

These results seem to indicate that satiated rats which have previously received food in the end box of an alley will reach this end box more quickly, when given an opportunity, than satiated rats which have never received food there or even been hungry while in the alley. This difference affects both the speed with which the rats actually run the alley and the time they spend in the starting box before starting on their run.

Discussion

The rats in the experimental group appear to be motivated to reach the end box, although presumably, they have no need of the food which they have learned to expect there. One could say that the "reward expectancy" built up in the runway on rewarded trials becomes "functionally autonomous" by operating even when basic physiological need is in abeyance.

Comparable results have been obtained recently in a number of experimental situations. Zeaman & House (1950) have shown that a habit that has led to the satisfaction of a light-avoiding drive will persist at quite a high level when the original drive is entirely eliminated. This result is especially interesting because a primary drive depending entirely on external factors can, of course, be far more convincingly eliminated than a vegetative drive like hunger. Using hunger, Miller (1948) reports almost errorless performance on first trials by satiated rats in a maze in which they had previously been rewarded. Webb (1949) found incidentally that a group of rats satiated for both food and water would still respond to a door behind which they had previously found food pellets.

The question immediately arises of whether we are not in all these cases dealing with examples of habit persistence; acts which have been repeated so often in a certain situation that they run off mechanically even in the absence of the original motivation. Hull has suggested that any drive may activate any habit, so that in the present experiment the strong running habit of the experimental group may be activated by another, "irrelevant," primary drive, even though the hunger drive has been satiated. This effect would not be observed in the control group, which has had no previous reinforced trials and so no habit that can be activated. In order to decide between this explanation and one in terms of learned elements in motivation further experiments are necessary. To begin with the motivation hypothesis would lead us to expect an observable effect on consummatory as well as on instrumental behaviour. If the experimental situation does indeed regenerate an eating drive in the satiated rat, then it should show some tendency to satisfy this drive apart from running to the food box; it should in fact eat when confronted with food there. In order to test this the second experiment was performed.

EXPERIMENT II

The subsidiary purpose of this experiment was to determine whether an external situation acquired the property of evoking a secondary drive because of its previous association with the primary drive state or whether a consummatory act in the situation was also necessary. This problem is of some importance from the point of view of systematic learning theory, as the rival principles of "contiguity" and "effect" are involved.

Procedure

The apparatus and general method of conducting each trial were the same as in Experiment I. Ten male rats of the same strain were used. The actual schedule for the experiment was as follows. On the first four days of the experiment all 10 rats were given daily 10-minute periods of free exploration in the runway after receiving their daily food ration. In this way they were made familiar with the apparatus, without any association of either hunger or food with the experimental situation.

On the fifth day of the experiment the animals were divided into an experimental and a control group of five rats each. The experimentals were given one rewarded run daily for the next 12 days, while the controls were run non-rewarded for the same number of trials. All runs took place about half an hour before the daily feeding period. On the succeeding five days both groups were given one run daily immediately after the feeding period, a reward pellet being provided in the end box for both groups. If the animals did not eat this pellet within one minute of reaching the goal box, they were taken out of the apparatus. Running times were taken for both groups.

RESULTS AND DISCUSSION

Of the five animals in the experimental group, four consumed the pellet of food provided in the end box on all five satiated trials, within a few seconds of reaching the end box. The fifth rat in this group showed the same behaviour on the first two of the satiated trials, but did not eat the pellet within one minute of reaching the end box on the last three trials. Of the five animals in the control group, which had never eaten in the end box, four never touched the food pellet on satiated trials, the fifth consumed the pellet on one of the five trials. These results are not amenable to statistical treatment, because the number of subjects is so small, but they are certainly suggestive.

Not only did the two groups show differences in their reaction to the food provided in the end box, they also differed with respect to the speed with which they approached the end box as in Experiment I. The running times obtained on satiated trials were converted to their logarithms and the performance over the five trials was then averaged for each rat. It was found that the difference between the mean running times of experimentals and controls was significant at the 5 per cent. level.

Thus the behaviour of the experimental group appears to show evidence of the continued action of some sort of hunger drive after satiation in the home cage, while the behaviour of the control group does not. This drive seems to show itself both in the tendency for a consummatory reaction to take place in the end box and in the increased speed in getting there. The previous rewarded runs seem to have led to an "externalization" of drive, whereby the cues from the apparatus became able to arouse a certain residue of drive even in the absence of organic need. Also it seems that this effect definitely depends on previous experience of reward in the situation to which the drive is "externalized"; the mere presence of unsatisfied drive, as in the control group, has no such effect.

It is apparent that the significance of the present experiment depends on the assumption that the primary drive is at a minimum immediately after cessation of the consummatory act of eating. It is important to remember that we are dealing here with the behavioural effects of the drive and that these may not be directly correlated with the retoration of physiological equilibrium. Behavioural studies do indeed show quite clearly that primary drive action is at a minimum immediately after the completion of consummatory activity and not at some time afterwards. The best empirical study is that of Koch and Saltzman (1948); Zeaman & House (1950) in a recent discussion of the general problem of the zero point for primary drives quote a number of references.

But the results of this experiment can hardly carry conviction as they stand. Quite apart from the small numbers involved, everything depends on the efficiency of the satiation technique employed in really eliminating any general need for food. If both groups were under some residual drive when starting their test runs then the differences in their behaviour could be accounted for by the fact that the control group had never previously fed in the apparatus and so would have to be under a strong need to "take the plunge" and eat in this unusual situation. Moreover, their reluctance to eat would be enhanced by the fact that they had had no previous experience with the pellets provided in the end box. It is thus necessary to check up on three points: the satiation technique, the effect of fear on failure to eat in a relatively unfamiliar situation and the effect of strangeness of the reward object on consummatory behaviour.

The last point was readily cleared up. The control group of six rats used in Experiment I had been given 17 non-rewarded runs in the apparatus on the same days as the animals used in the present experiment. On the 18th day they were fed by hand liberal quantities of reward pellets in their home cages with their daily food ration. After satiation they were run in the apparatus and provided with a pellet in the end box. None of them consumed this pellet within one minute. As a further test the pellet was presented to them by hand for another two minutes while they were in the end box. There was much sniffing but no eating of the pellet. It appears then, that the results of the first experiment were not due to the lack of familiarity with the reward object on the part of the control animals.

EXPERIMENT III

This experiment was designed to determine whether the eating behaviour shown by satiated rats in a place where they have previously been regularly rewarded is due to a defective satiation technique, and whether the failure of satiated rats to eat in a new situation is due to fear of eating in a place in which they have never eaten before.

The satiation technique used consisted in daily feeding periods on rat-cake, but on the test day, each rat, after having finished eating this, was fed experimental reward pellets by hand until it repeatedly rejected them. Then it was immediately given its test run with a pellet in the end box. If it ate this pellet, when it had just refused an exactly similar one in the home cage, this could hardly have been due to defective satiation.

Most rats are shy about eating in an unfamiliar situation. But when they have once been coaxed into eating in a place, they will always do so again spontaneously on the next occasion. Hall (1936) has shown that a sharp drop in emotional behaviour occurs when animals have once eaten in an unfamiliar situation. The control group in the present experiment was therefore given a single rewarded trial when hungry, a few hours before the test trial. Their single experience of eating in the end box should have removed their inhibition about doing so, and so if they still did not eat on the satiated trial, this must be simply due to the effectiveness of the satiation procedure. But a single eating experience in the end box must have begun the process of "externalization," and so the results of this group would probably not be quite as negative as those of the control groups in the first experiment.

Apparatus, details of procedure, preliminary training, feeding schedule and reward pellets were the same as in the first two experiments. The subjects were 17 male rats, 9 experimentals and 8 controls, of the same strain as those used in the previous experiment and about four months old at the start of the experiment. After four days of ten-minute exploration periods in the runway, all the animals were given three runs a day for the next three days. This method was adopted to save time. Both groups were run 15–30 minutes before their daily feeding period; the experimentals found a food pellet in the end box, and the controls were removed from the empty end box after 30 seconds. There was a five-minute interval between each of the daily trials. This was spent in a small, special cage kept for this purpose. On the next day, the test day, the procedure was the same for both groups. There was one rewarded trial when hungry, then the satiation procedure already described, and finally the test run immediately afterwards. The animals were given one minute to consume the pellet provided on this run, and if they had not begun eating at the end of this period, they were gently offered the pellet by hand while in the end box for another two minutes. At the time of the test-run the experimental group had therefore had 10 previous rewarded trials, while the control group had had nine non-rewarded and once rewarded trial.

RESULTS AND DISCUSSION

All the nine rats in the experimental group consumed the pellet on the test run, but only three out of eight in the control group did so. These results can be drawn up in a 2 × 2 table and treated by Fisher's exact method. This gives a probability just less than 0·01. Applying Yate's correction the table also yields X^2 value of 5·24 which gives a P value just above 0·02. The difference in the eating behaviour of the groups may therefore be taken as statistically significant.

Testing the difference in the mean running speed of the two groups on the test trial, a value of 2·85 is obtained, which at 15 d.f. is significant at the 2 per cent. level.

The results of Experiment II both with regard to the consummatory reaction and the instrumental running activity have therefore been confirmed. That rats which have just rejected a food pellet in their home cage will run much quicker than satiated rats normally do to a box where they know another such pellet to be and then eat this pellet suggests that the eating drive can be aroused by specific situations. It also suggests that the effects of such situations are surprisingly independent of one another, so that satiation in one does not make a fresh arousal in another situation impossible.

The fact that as many as three out of eight animals did eat on the test trial is perhaps surprising. It will be remembered that all eight rats had eaten in the experimental situation once before, so that there could be no question of a feeding inhibition due to lack of familiarity. We can only conclude that either the first rewarded trial is rather more important than any succeeding ones in setting up "externalization" effects, or else that "externalization" needs only very few trials altogether. This can only be tested by further experiment. General considerations about the shape of learning curves and the rapid learning of determining sets at the beginning of the curve make both these possibilities likely ones, however.

A number of investigators have reported eating or drinking by rats satiated for food or water. In considering their results it is important to remember that eating and drinking like any other consummatory act, may appear as "displacement activities" (Tinbergen, 1940), when another drive is strongly aroused while its own consummatory act is not possible in the situation. Eating or drinking behaviour in satiated rats can only be taken as evidence for the operation of an acquired situation—specific drive if there are no other strong drives operating at the time. An experiment performed by Miller (1948) provides some interesting data on this point. In order to demonstrate displacement drinking two groups of rats were trained to run to a water reward in a runway similar to the one used in the present investigation. One group was then run satiated for both food and water and another group was run hungry but not thirsty. There was some drinking at the end of the runway by the first group, but much more by the second. The difference between the amounts drunk by the two groups is due to the displacement factor, but this does not account for the drinking by the first group, which was presumably due to a situation-specific thirst drive. Drew (1937) has also shown that satiated rats will eat (*a*) in a place in which they have previously eaten, and (*b*) in certain situations which seem to involve conflict for the animal. By the present interpretation we get eating due to a situation-specific eating drive in the first case and displacement eating in the second.

EXPERIMENT IV

INTRODUCTION

In all three experiments reported so far the behaviour of the rats on satiated trials has been very similar to the behaviour they displayed on previous rewarded trials. The rats in the experimental groups have undoubtedly learned something which the controls have not, and this results in their different performance on satiated trials. But what is it that has been acquired? Is it a tendency to repeat mechanically all the actions of the rewarded trials irrespective of primary motivation, or is it some dynamic set specific to the situation?

In order to answer this question, the preliminary running to the end box of the apparatus was separated from the obtaining of reward there. The experimental animals were at no time run to an end box containing food, so that if they were to run faster on satiated trials, this could not be due to the persistence of the running habit. Nor could it be due to the sensitizing of a habit by an irrelevant drive, because no opportunity was given for any habit to form. If one now gives food to the satiated rats in the box in which they have previously been fed, will they eat it? If they do, will they still run to that box faster when given an opportunity immediately afterwards?

Procedure

The apparatus and control of feeding and satiation were the same as in Experiment I. Fourteen female rats of the same strain were used as subjects. Running time and latency of response were measured in the same way as before.

On each of the first four days of the experiment the animals were allowed to explore the apparatus freely for five minutes, a short while before their daily feeding period. On the next five days they were each given a single non-rewarded run daily when hungry. The purpose of this nine-day preliminary training period was to allow the animals to familiarize themselves thoroughly with the runway. On the 10th day the animals were divided into two groups of seven rats each. The end box was shut off from the rest of the apparatus by a sliding door. About an hour before their daily food ration was due each of the 14 rats was placed directly in the end box and allowed to remain there for two minutes. About 20 minutes later they were put into the end box for another 40-second period, and 20 minutes after that for a final period of 40 seconds. A little while later they received the daily food supply in their home cages. The only difference between experimental and control groups was that the former were allowed to consume a quantity of food in the end box while for the latter the end box was empty. Thus the end box was associated with reward for the experimental group, but that reward had never been preceded by a run down the alley. At the end of the usual daily feeding period each of the animals was once more put in the closed end box for a period of 40 seconds. On this occasion too, the end box was empty for the controls, but contained food for the experimentals. At the end of the 40-second period the animal was lifted out, the sliding door was removed, the animal was taken to the starting box and was allowed to run down the alley in the usual way, its time being measured. The run led to an empty end box for both groups.

On the 11th, 12th, 13th and 14th days of the experiment the procedure was to give each animal a 40-second period in the closed end box about 30 minutes before the daily food supply, and then to give them one run immediately after satiation, without any further stay in the end box. The end box was always empty for control animals, but contained food when experimental animals were introduced for their 40-second stay. It never contained food on satiated runs.

RESULTS

As in the first experiment the two response measures, running times and latency of reaction were converted to their logarithms and mean log. values were calculated for each trial. Table IV gives these mean values for the running speed on the last six days of the experiment. As in Experiment I, log. values are multiplied by 1,000 to eliminate decimals.

TABLE IV

1,000 X MEAN LOG RUNNING TIME ON LAST SIX TRIALS

	Days							
	9	10	11	12	13	14	5–9	11–14
Experimentals	1,158	1,091	760	1,013	972	918	981	917
Controls	1,084	981	1,255	1,148	1,334	1,194	1,044	1,233

The ninth day, is the last day on which both groups are run hungry and without reward. It marks the end of the preliminary period of familiarization. On all subsequent days the animals are run satiated, but the experimentals have been receiving short periods of feeding in the end box. The procedure on day 10 is different to that on other days in that a period in the end box immediately precedes the satiated run. For the experimental group the end box contained a pellet of food on this occasion as usual. Now, although they had just refused any more food in their home cages, all seven animals in this group proceeded to eat the pellet available in the end box. It will be seen that their subsequent run was rather slower than any later run, when this extra pellet was not provided. Using the method for correlated samples, the difference in performance of the experimental group on days 10 and 11 yields a t value of 6·04 which makes $P < 0{\cdot}001$. The difference between the two days is therefore not likely to be due to chance fluctuation. The last two columns in the table give the average values for the five preliminary hungry non-rewarded trials and for the last four satiated trials for each group. The difference between running speed during hungry and during satiated trials are significant for neither group.

TABLE V

SIGNIFICANCE OF DIFFERENCES BETWEEN MEAN LOG RUNNING TIMES OF EXPERIMENTAL AND CONTROL GROUPS ON VARIOUS DAYS AND GROUPS OF DAYS

	Days					
	9	10	11	5–9	11–14	12–14
t ..	0·287	1·76	2·73	0·073	2·64	1·99
P ..	<0·8	$0{\cdot}1^{+}$	<0·02	>0·9	$0{\cdot}02^{+}$	<0·1

For 12 degrees of freedom a *P* value of 0·1 actually requires a *t* value of 1·78 and a *P* value of 0·02 requires a *t* value of 2·68.

The comparison between the average performance on days 5–9 and on day 9 separately shows that there was no significant difference in the performance of the groups before they were subjected to different experimental treatment. It is to be noted that the difference on day 10 is in the opposite direction to those on the following days, i.e. on this day the controls are faster.

Similar comparisons using reaction latencies instead of running times give results that lie in the same direction, but are less significant statistically. However, the difference between reaction latencies of experimentals and controls on day 11 is still significant at the 5% level.

What must be explained, therefore, is the fast performance of controls and the retardation of experimentals on the first satiated trial, the extremely fast performance of experimentals on the second satiated trial, and the persistent rather faster performance of experimentals on subsequent satiated trials.

DISCUSSION

A possible interpretation of the difference between the groups on days 11–14 is that the periods of confinement in the closed, empty end box strengthened any fear reaction which the control animals may have had initially to this part of the apparatus. This would cause them to approach the end box more slowly on satiated trials than the experimental animals. The activity level of satiated rats is, however, well below that of hungry rats, and an unpublished experiment of the writer's showed that hungry rats will always run faster in this type of apparatus than satiated rats, even though neither group receives a reward. What must be explained in the present experiment, therefore, is not why the controls tend to run more slowly on satiated trials, but why the experimentals fail to do so. In any case, if the control animals had acquired a fear reaction to the end box, this would show in their score on the first satiated trial (day 10), because the run was only taken as completed if the animals had three legs actually inside the end box. But in fact five of the seven animals in this group actually entered the end box more quickly on day 10 than on day 9 before they had ever been confined there.

So far as the main object of the experiment is concerned, there is no evidence for the formation of a "running habit" on days 5–9. The mean time score on day 9 is actually well above the mean for all five days, so that if anything, the animals are slowing up as they get completely familiarized with the runway. Their tendency to run to the end box much more quickly than the controls on day 11 and rather more quickly on the following days is therefore not due to the persistence of a running habit, whether sensitized by an irrelevant drive or running off mechanically. A possible explanation of the faster running times of experimental animals on satiated trials might be given along the following lines. The animals presumably have some memory of feeding in the end box, which is probably related to an expectation of feeding there again. Now what seems to happen is that this expectation acts like a motive in that it activates behaviour which is likely to lead to its satisfaction. In the present setting the animal makes use of previously acquired information about the runway in an attempt to bring about the satisfaction of its expectation of being fed in the end box. That this can occur in a hungry animal has already been shown by Seward (1949), who first allowed rats to explore a single unit T-maze thoroughly, then fed them in one end box and immediately afterwards gave them a free run from the starting box. Almost all the animals ran to the side on which they had just received food. Seward's animals were still hungry when making their choice, but in the present experiment the reward expectation seems to be operating independently of the primary drive. This too, is not entirely unknown, for MacCorquodale & Meehl (1949) found that rats satiated for both food and water would gradually prefer that side of a single unit T-maze on which these incentives were provided.

The effect we have been discussing only appears in pure form on the 11th day of the experiment. On subsequent trials it is much less well marked because the animals are presumably acquiring a discriminative expectation as a result of being repeatedly *not* fed when reaching the end box at the end of a run. Presumably, whereas before the expectation was simply, "food in end box," they are now beginning to distinguish between "food when put in end box," and "no food when running to end box." The fact that differences between groups are less well marked on reaction latencies than on running times is probably due to the difficulty which the rats have in distinguishing the starting box from the end box. Before they learn to react immediately to the brightness difference, the experimental rats probably spend some time looking for food in the starting box and this will increase their latency score.

The results of the 10th day must now be considered. On that day the experimental animals all ate the pellet of food provided in the end box, even though they were supposedly satiated, and showed a rather retarded performance immediately afterwards. The eating behaviour is in line with the results of Experiments II and III, and incidentally, it shows that it does not depend on a previous run which may have built up an energy debt. It is apparent that the end box instigates eating behaviour irrespective of what eating may have taken place elsewhere. It is also clear that those situations which are capable of instigating eating behaviour are situations in which the animal has fed previously; they are the situations too, in which, as we phrased it above, it expects to feed again. In other words, expecting to eat and wanting to eat are equivalent here, and a "reward expectancy" seems to be acting like a highly specific motive. The wider theoretical implications of these views will be discussed elsewhere (Danziger, 1951).

What happens on day 10, therefore, is that the specific reward expectancy about feeding in the end box is satisfied before the run and so cannot act as a motive. The animals therefore run, if anything, more slowly than the controls. It is possible that there may actually be an inhibitory effect involved, but this will require more experimentation. In any case, the views put forward in this discussion are at present only tentative and await further experimental exploration and confirmation.

CONCLUSIONS

The behaviour of satiated rats seems to indicate that when rats have had some experience of eating in a certain situation, the reward expectancy connected with that situation is able to operate in the absence of any physiological hunger drive in such a way that the animals display the sort of behaviour that would lead to the satisfaction of the reward expectancy in a constant environment.

Thus when a tendency to eat is aroused by the external situation it acts like an internally aroused drive in that it activates learned instrumental acts and utilizes stored information. Satiation of a primary drive in one situation does not make it impossible for a different situation to arouse this drive again. But only situations associated with a consummatory act and not those associated with an unsatisfied need state acquire the capacity to rearouse the drive in the satiated animal. This suggests that the process may be one of a reward expectancy becoming "functionally autonomous."

The consummatory action appropriate to the primary drive is important for the continued operation of the secondary drive. In this way the acquired drives of rats seem to differ from those of men.

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THE TIMING OF RESPONSES TO SPATIAL PERCEPTION QUESTIONS

BY

V. R. CANE and V. HORN

(From the Medical Research Council Applied Psychology Research Unit, Cambridge)

EXAMINATION of the time of response to single problems in spatial perception suggests that the approach of subjects to multiple-choice questions may differ from their approach to free-choice questions. There is some evidence for thinking that the slower subjects score more highly, and that practice in a given type of problem affects speed rather than correctness of response. These results may provide a new interpretation of some effects previously observed in repeated retesting of the same group.

The design of the experiment discussed was a "balanced incomplete block," in which 120 subjects were required to answer 108 out of the total of 540 problems.

I

AIMS OF EXPERIMENT

This research was originally undertaken as a basis for preparing a number of parallel tests of spatial perception. We wish to have such a group of tests to enable us to investigate, for instance, the effects of practice and of certain sorts of teaching. These enquiries would require the testing of the same subjects on more than one occasion, and there are obvious objections to using the same test repeatedly.

The Shapes Analysis Test, consisting of six types of questions each requiring some sort of mental manipulation of shapes, was prepared. There were 540 questions in all, 90 of each type, and our aim was to sort them out into a number of equivalent sets. We decided that two questions could be regarded as equivalent if they were of the same type, if the same number of people in a given population answered each correctly, and if the average time spent on each was about the same. This last point has not, we think, been considered before but is clearly of some importance where tests given with a time limit are concerned. It was necessary, therefore, to devise some method of recording not only the answer of each subject to each question, but also the time he spent on it. The apparatus constructed for this is described in detail later.

Since the time records were necessary in any case, we decided that we might in the same experiment make some preliminary enquiries into the relation between the speed and score of subjects, and between the difficulty of a question and the time spent in doing it. This did not in fact make the experiment more complicated, as will be seen from the discussion on the experimental design. We also gave the subjects an intelligence test so as to be able to compare their score on this with their score on the Shapes Analysis Test.

II

METHOD OF EXPERIMENT

1. *Subjects*

We chose as our subjects 13- and 14-year old children at a secondary modern school. Since it is known that the ability to deal with this sort of visual material depends on the age and sex of the subject, it seemed reasonable to take equal numbers of each sex and of two age groups from a population that was in other respects fairly homogeneous. We therefore took four batches each of 30 children, 13-year-old boys, 13-year-old girls, 14-year-old boys and 14-year-old girls, avoiding as far as possible children from the "C stream," who were the worst by the school's standards.

2. *Questions*

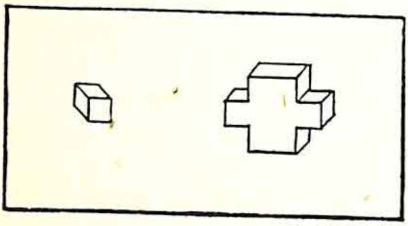
Of the six types of questions (called A, B, C, D, E and F), A, B and F were three-dimensional though represented in a plane, and C, D and E were two-dimensional. A and C required the subject to supply his own answer whereas D, E and F offered a choice of five

FIGURE 1

EXAMPLE QUESTIONS

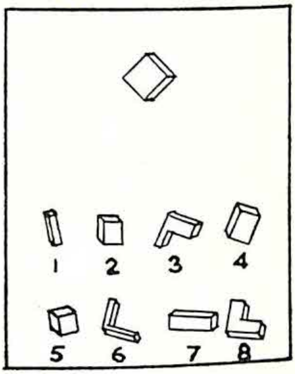
THE SHAPES MAY BE TURNED IN ANY WAY.

A 3-dimensional



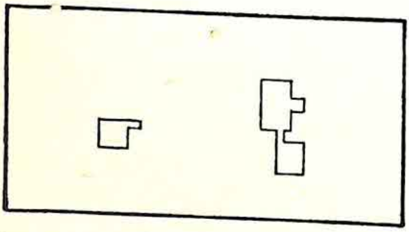
How many units like the small figure are needed to make the large figure?

B 3-dimensional



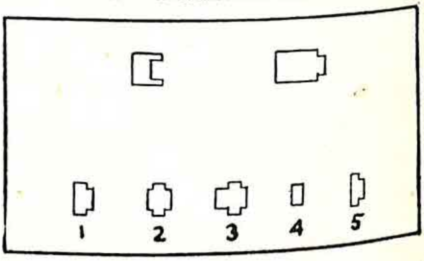
Which two of the eight lower figures are needed to make the figure above them?

C 2-dimensional



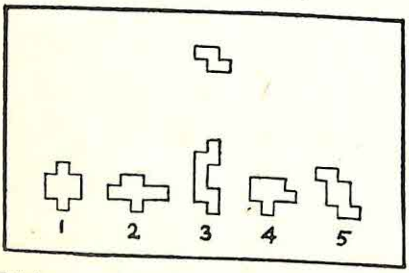
How many units like the small figure are needed to make the large figure?

D 2-dimensional



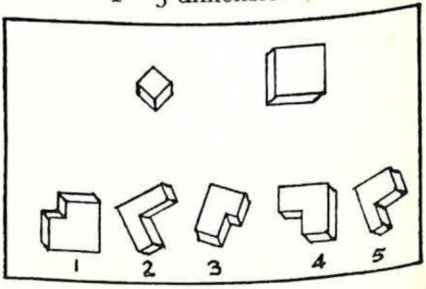
Which one of the lower figures will remain if the top left figure is taken from the top right figure?

E 2-dimensional



Which one of the five lower figures cannot be made from a number of units like the figure above them?

F 3-dimensional



Which one of the lower figures will remain if the top left figure is taken from the top right figure?

answers. B offered a choice of 28 alternative answers, and therefore it was considered to be more like A and C than the multiple-choice ones, D, E and F. The questions of each type were divided into five sets of eighteen each, balanced in difficulty as far as we could guess. The sets were called A1, A2, A3, A4, and A5; B1, etc. to F5. Each subject did six of these sets, two from each of three types, making 108 questions in all. The subject had to do examples of the relevant type of question before embarking on the test proper. These examples (some of which are shown in Figure 1) are simple illustrations of the principles concerned. The test questions themselves are in general much more difficult, as we aimed to produce a test of a very high level.

3. *Timing and Answer Recorders*

The Timing Recorder was made in the laboratory workshop. It is an ink-writing polygraph with seven pens, each controlled by a solenoid which, when energized, causes the ink line on the paper to move sideways about an eighth of an inch. One pen is used as a time marker and plots time intervals every five seconds. Each of the remaining six pens is connected to one of the Answer Recorders, thus allowing six subjects to be tested simultaneously.

The answer sheet completed by a subject is in the form of a roll of paper with small rectangular spaces (¾ in. × 1½ in. and ½ in. apart) printed along the length of the paper. Each space is numbered serially to correspond with the question numbers, and the subject is asked to write his answer in the appropriate space.

The roll of paper is inserted in an Answer Recorder which has an aperture through which the subject can see only one numbered space at a time. When the subject has answered a question, he at once turns the knob at the side of the machine and so brings the next blank answer space into view.

A star wheel fixed to the same shaft as the knob operates a contact so that all the time the knob is being turned the circuit to the solenoid, controlling one of the "Answer pens," is being alternately made and broken, causing the pen to move to and fro across the paper of the time recorder. Thus the turning of the knob on any Answer Recorder will be registered by the movements of the appropriate pen, and at all other times the pen will be recording a straight line. The length of straight line between each group of to-and-fro marks is therefore a measure of the time spent on each question.

4. *Testing Technique*

The 120 schoolchildren were tested in groups of six. General instructions were first given to each group of subjects on the use of the Answer Recorders. No mention was made of the Time Recorder. Subjects were told that an answer was to be given to every question, but if, after considering a question for some time they were still uncertain about the answer, they should guess rather than omit it. It was emphasized that there was no time limit and it was clear that the children did not realize that they were being timed on individual questions.

Each child was given a book of questions and the instructions for that particular series—the first 36 questions of the 108 he was to answer (in accordance with the plan of the experiment). In addition to understanding the first two examples for which the answers were supplied, subjects had to answer correctly two other examples before starting the test proper. Individual assistance was given when necessary to ensure that every child understood the principle of the type of question he was to answer. The period of completing the example questions was useful not only in providing practice with the questions, but also for some experience in the manipulation of the Answer Recorder.

After answering the 36 questions of the first two sets, the children were given a rest and drank their mid-morning milk. They then completed the second lot of questions after which they stopped for another break and school dinner, and in the afternoon answered the third book of 36 questions. Each child spent one complete school day taking part in the experiment.

III

THE EXPERIMENTAL DESIGN AND THE ANALYSIS INVOLVED IN IT

Since there are six types of questions and 90 questions of each type, and we wished to compare questions of the same type in respect of difficulty and time spent on them, it was clear that a somewhat complex experiment was necessary. It is not enough to give the 90 questions several times over in the same order as some positions may be more favourable than others—e.g. late in the series if there is a learning effect, or early in the series if there is a fatigue effect. Moreover there may be transfer between individual questions. Ideally we should like to give the questions of one type in all possible orders, or at least so that each occurs equally often in each position; but even if we could do this, we should still have no means of comparing the difficulty of different types of questions, which is interesting if not essential to our immediate aim, nor could we make allowance for the transfer effect which may occur between types of questions, a matter which would need attention at some stage in the construction of the parallel tests. It was therefore decided that in this experiment each subject should do a series containing more than one type of question, that each question should be done an equal number of times and with an equal chance of being in any particular position in the series, and that each pair of types should be done equally often together in a series.

It was thought that a convenient number of questions for each subject to answer was 108, 36 from each of three types. We arranged thirty sets of 18 questions, as explained before, and each subject did six of these, two from each type, e.g. A1 A5, B2 B3, C4 C2. (See Experimental Design in Appendix.) To begin with, these sets, rather than individual questions, were treated as the units. The subject did two sets from each type so that practice effect could be observed directly, that is a comparison of the performances of the same subjects on the same type of questions could be made; they were done in immediate succession so as to cause as little interruption in the experiment as possible. The questions within a set were randomised by means of a random number table each time they were used. In this way, each question had an equal chance of appearing in each position in the set, and the design of the experiment ensured that each set appeared equally often in each position in the series. Also questions of type A, for instance, were preceded equally often by each other type, and were done equally often in each position by each batch of subjects. It was thus possible to assume that the total scores on different questions were comparable in respect of learning or fatigue effect, effect of transfer from one type to another, and effect of age and sex of subjects; that is, two questions of equal difficulty would not appear to be different merely because one was more often done late in a series, or by boys, or after some other type of question that was particularly favourable. It was still possible, however, that the difference between two questions in different sets would be affected by their being done by individuals of very unequal ability and an adjustment was made for this (see Appendix). Apart from this adjustment between sets of questions of the same type, all the estimates of difficulty of questions whether individually, or by types, were made from the total number of right answers to the question. The ability of the subject was estimated by his total score, with some correction made for the difficulty of the questions he had to do. The adjustments for difficulty of questions and ability of subjects were not, of course, independent, but were suitable as first approximations.

A slightly more complicated method was necessary in dealing with the time spent on questions. In the first place it was clear that the time scores were by no means normally distributed; it was therefore decided to work with the logarithm of the time. (When the histogram of this was plotted, it was found to be symmetrical and reasonably like normal.) The second difficulty was that no experimental design could arrange for balance in respect of right or wrong answers, which could appear from any subject, to any question, in any position in the series. Therefore before making a comparison between time taken by a subject and his score, or the time spent on a question and its difficulty, we wished to discover whether there was any difference in the time taken to give a right answer and to give a wrong answer. If for instance it were quicker in general to get a test question wrong, perhaps because less attention was paid to studying it, then the subject with high scores would be slower because he had more right answers, and the difficult question would be done more quickly because it had more wrong answers; and yet it would be misleading to say that the good subject was an intrinsically slow worker or that the hard questions took less time. Thus it was necessary to work out whether there was any consistent effect of this sort before looking for other effects concerning the relation of score and time. We therefore considered separately each set of eighteen questions and each of the four groups of children doing them and, after making an adjustment for the difference in performance of the subjects and difference in average time of the questions, compared the total time spent on right answers with the total time spent on wrong ones. There was very rarely any significant difference between these two times, and as the effect was not consistent, the right answers sometimes taking less and sometimes more time, and as also the average effect, for each type of question, was insignificant, we felt that we were justified in neglecting even the significant cases.

The rest of the analysis of the time measures could then be made in the same way as that of the scores.

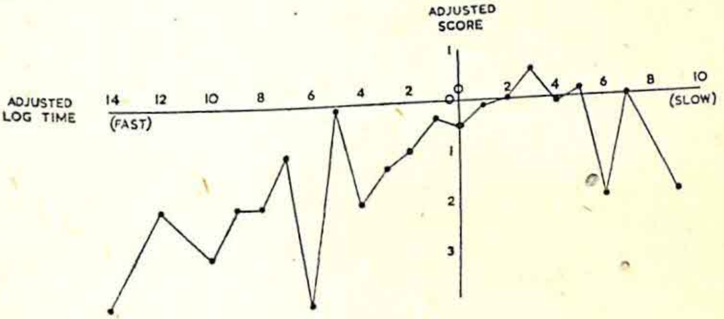


FIGURE 2

REGRESSION OF SCORE ON TIME (AVERAGE SCORE OF SUBJECTS TAKING THE SAME TIME)

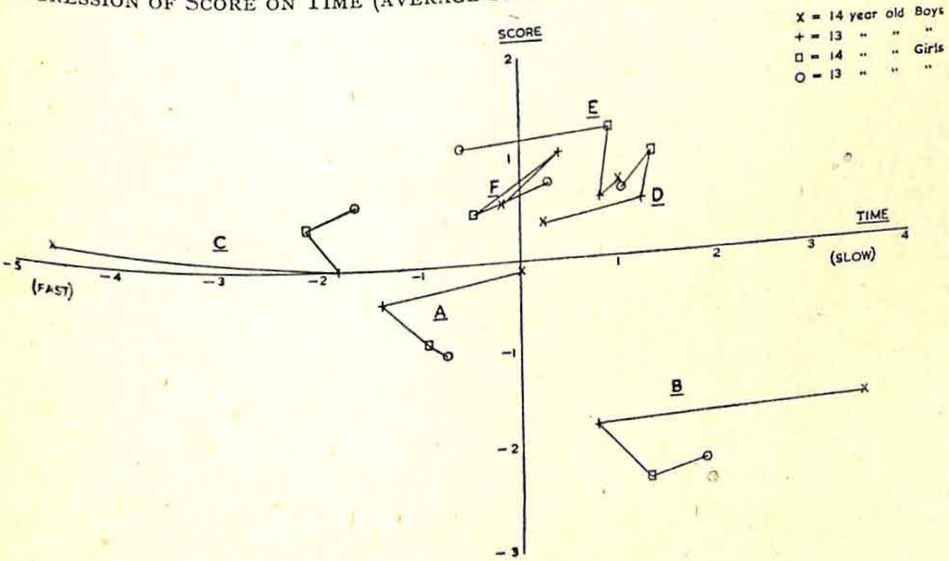


FIGURE 3

RELATION OF LOG TIME TO SCORE FOR QUESTION TYPES

IV

RESULTS

At the end of the analysis we had the following estimates:

- (i) The score and speed for each subject,
- (ii) the difficulty (within its type), and the mean time taken, for each question,
- (iii) the relative difficulty of, and time taken by, the different types of questions,
- (iv) the total effect of practice on score and on time, not distinguishing between types of questions,
- (v) the time and score for batches of subjects of different age and sex.

(i) and (ii) were plotted on scatter diagrams, which are not given here. The regression line of Figure 2 summarises the relevant information about the relationship of score and speed of subjects, and a set of two by two tables (see Appendix, Table I) shows how far questions which are above or below average difficulty take more or less than average time.

The scatter diagram for (iii) is given above (Figure 3). For each type of question, and each batch of subjects, the average number or correct answers is plotted against the average log-time taken. Thus there are four points corresponding to each type, and these are linked by straight lines so that the difference between types can be readily seen.

On the results of (iv) and (v), two analyses of variance were performed, one for score and one for time (see Appendix, Tables II and III).

It was found that the position of a set of questions did not affect its score, i.e. there was no fatigue or practice effect. There was, however, not only a significant difference between the scores of boys and girls, and between 13- and 14-year olds, as was expected, but also a significant age and sex interaction. In fact from the following table of total scores,

	Boys	*Girls*
13-year old	1040	786
14-year old	1095	979

it can be seen how much more girls improve than boys between 13 and 14.

The time spent on a set of 18 questions was significantly affected by its position, in fact the time decreased by about one-tenth on the average between one position and the next. The drop was particularly noticeable between positions 1 and 2, 3 and 4, 5 and 6, when subjects were doing two sets of the same type, but there was also a drop from positions 1 to 3, and 2 to 4, so that there was a general, as well as a particular, practice effect on *speed*. There was again a difference between boys and girls (boys being slower), but no significant age difference.

It should be pointed out that the test questions were very difficult for the children; the scores were generally less than half the possible maximum and for type B questions many of the scores were zero. This was not surprising, since the test has presented considerable difficulty even to University students.

V

DISCUSSION

The most interesting results of this experiment are those involving the time measurements, for, so far as we know, very few systematic enquiries have been made into the relation of time taken and correctness of answer, and those that have been undertaken have generally been for a rather different purpose. For instance, Peak and Boring (1926), McFarland (1930) and Slater (1938) have all shown that subjects tend to be consistently fast or slow in their responses, whereas we confined ourselves to considering the average rate of response of a subject. Macleod (1929) concludes that the level of difficulty of a task affects the correlation between speed and accuracy; we have only concerned ourselves with the relation at a constant level of difficulty, and that rather a high one. If what we have found with this particular series of tests should prove to be true with all sorts of tests, many of the effects which till now have been expressed in terms of score—as, for instance, that on timed tests the better subjects, in certain circumstances, improve more with practice than the poorer ones—may more satisfactorily be expressed in terms of time, and it may even be possible to connect time spent with some set of decisions that have to be made between alternatives. For example, when we investigated whether it took longer to get an answer right than to get it wrong, for similar subjects on questions of similar difficulty, we found that, although the differences in time were insignificant for each type considered separately, on the average it did take longer in the case of multiple-choice questions and less time in the case of questions to which the subject had to supply his own answer. (In fact, when the results for questions of the latter type were pooled, correct answers took *significantly* less time than incorrect ones. The effect, however, was small, the ratio of the two times being about 1·06.) This would agree with the idea that in the multiple-choice case the right answer would more likely be found if all the alternative answers were considered, whereas in the free-choice case the right answer would be found when the subject was offering himself a set of reasonable possibilities only, successfully limiting his field of enquiry to cut out unprofitable alternatives. It should be possible to devise experiments of a kind other than the paper-and-pencil test to explore this.

Two results which are most interesting, especially when considered together, are (i) that practice considerably diminished the time spent on such a test (in fact, by nearly 40 per cent. between the first and sixth sets of eighteen questions), but had little if any effect on the score, and (ii) that the better subjects tended to have a lower average speed. If this applies in general (at least with tests difficult for the group concerned), it means that when a test with a time limit is first given, the poorer subjects will cover more of the test in the given time and so approach nearer to their maximum score than the better subjects do. If, now, the test is given again to the same subjects they will all tend to work faster, but the increase in the number of questions answered will be of greater benefit to the good subjects, who will get more of them right, so that the better subjects will improve their scores more than do the worse ones. This may possibly be the explanation of the results found in repeated retesting with the same timed intelligence test (Cane and Heim, 1950) for an examination of the graphs given there shows that

(i) the increase in the number attempted is at least 55 per cent. over ten testings, whereas the increase in ratio of correct to attempted is at most 16 per cent., i.e. the most important effect of practice is improvement in speed,

(ii) better subjects work more slowly than poor ones on a difficult test.

The existence of a time limit in such a test will thus tend to minimise the differences between subjects, but this can be partly offset by giving the subjects preliminary practice on a similar test. If, in addition, it should be generally true that it takes longer to get a multiple-choice question right than wrong, then the preferential treatment which a test with a time limit extends to the weaker subject would probably be even greater in a test composed entirely of multiple-choice questions.

The remark in the previous paragraph, that better subjects tend to work more slowly than poorer ones, needs a little modification. As will be seen from Figure 2 the relation is not linear, the very slowest subjects (at the right hand side of the graph) showing a decline in score. Similar effects have been observed when intelligence tests have been given without a time limit (Cane and Heim, *in preparation*).

Among questions of the same type we found that the hardest questions tend to take longer, though the relationship is not very strong even for questions where the subject has to invent an answer and is less so when he merely has to choose one (see Appendix). When the time and score for different types of questions are compared (see Figure 3) it can be seen that the multiple choice questions again fall into a separate category—they have scores above the average, take about average time, and are very like each other with respect both to score and to time. The other three types of questions, C, A and B lie in very different parts of the plane of the graph, and roughly lie on a line which suggests that the most difficult types have the most time spent on them. This again suggests that in material of this type at least, the discriminatory power of multiple-choice questions is much less than that of free-choice ones.

The boys scored higher than the girls on each type of question, the greatest differences occurring with the three-dimensional types, A, B and F, but the difference between the older boys and girls was much less than between the younger boys and girls, so that girls probably improve between 13 and 14 to a greater degree than boys. It has been found before (Harris, 1947–49) that the intelligence test scores of girls increase more from 13 to 14 than in any other year between 11 and 16.

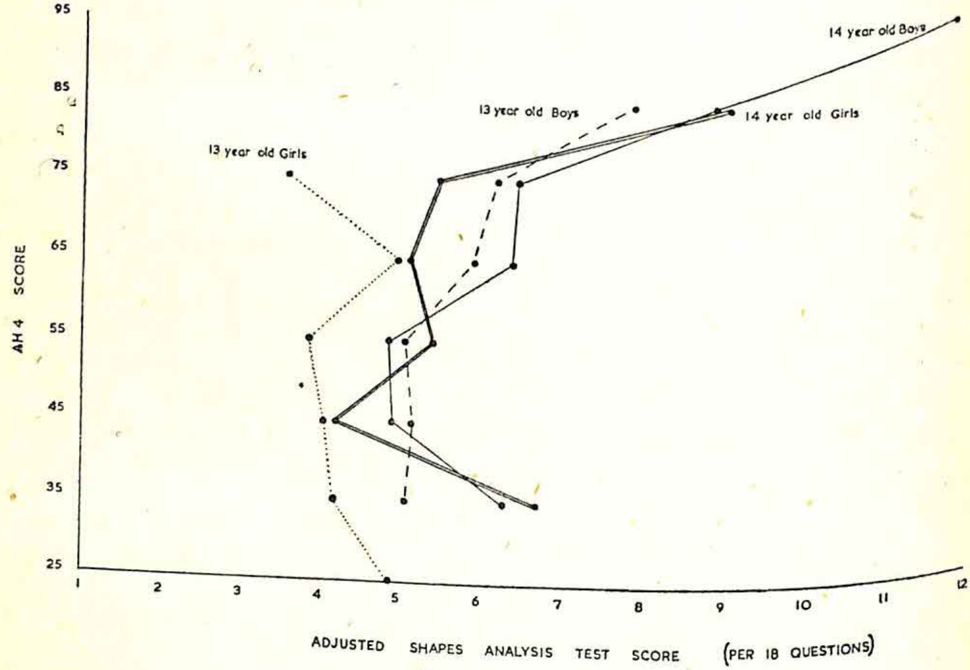


FIGURE 4

RELATION OF SHAPES ANALYSIS AND INTELLIGENCE TEST SCORES

There was a slight association between adjusted Shapes Analysis Test score and score on the intelligence test AH 4, the average correlation for the four groups of children being ·29. The distribution of AH 4 scores for these children was similar to that for an unselected industrial population except that the worst 10 per cent. of subjects had been omitted by the method of selection; for this reason we show also a regression line. It is clear from Figure 4 that the relationship is by no means linear, but that subjects scoring highest and lowest on AH 4 do rather better on the Shapes Analysis Test than would be expected from the trend in the middle of the group. The fact that the Shapes Analysis Test was so much too difficult for the weaker subjects, as well as some selection in the subject range, has undoubtedly reduced the correlation; when many of the scores are zero (as occurred especially with type B questions) there is no discrimination at all between subjects of medium and low ability, and their total Shapes Analysis Test scores will tend to be equal. This may account for the almost complete lack of relationship between the two scores in the middle of the group, but it cannot explain the relatively good performance of the six worst AH 4 subjects, nor can it explain why the younger girls score least on the Shapes Analysis Test at each level of score on AH 4. It is difficult indeed to give any plausible reason for this; but as the correlations between the two scores are greater for the older children than for the younger it definitely does not support the theory that there is an increasing differentiation of abilities with age. Altogether it appears that the relationship between our test and an accepted intelligence test is very slight; how far they differ because the two tests call upon different "factors" in the usual sense, or because one is given in limited time and the other is not, or because one is very much more difficult than the other, cannot as yet be decided.

VI

CONCLUSIONS

It is clear from the results of this experiment that there are time effects which would play an important part in determining the score of a subject in a timed test. Since the relation between performance on this test and on a timed intelligence test is very slight, it cannot be asserted that similar time effects would be observed with intelligence test material, but the nature of the results make it likely that it would be so, and it should probably be investigated.

In particular, it appears possible that the number of alternative responses provided for a question may influence the time spent on it, and that when there is complete freedom of choice the behaviour of the subject may be of an essentially different kind from what it is in a situation involving limited choice. This could probably best be investigated by observation of individual subjects, and we hope to do some experiments with various sorts of material with and without a time limit, and with and without instructions to guess an answer when in doubt. We shall moreover reconsider all these conclusions in the light of the results we get when we use the parallel tests prepared from these questions.

We wish to express our thanks to Dr. A. W. Heim, who has given us every possible support in this experiment, to Capt. A. M. N. Aston, M.C., and his staff and pupils of the Eldon Road Secondary Modern School, Edmonton, who so willingly co-operated in the actual testing, and to Mr. F. J. Anscombe of the Statistical Laboratory, Cambridge, who both produced the experimental design and gave us most valuable advice at every stage of the analysis of our results.

VII

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STATISTICAL APPENDIX

Design of the Experiment

	13-year-old Boys			*14-year-old Boys*			*13-year-old Girls*			*14-year-old Girls*		
1	E45	C52	D43	F35	B51	C25	F32	A25	C23	F31	D15	B41
2	E12	A13	C32	A41	C34	B25	D21	A42	C12	F42	C43	A24
3	F12	B52	E54	C52	B12	A53	D43	B42	C34	A53	F15	B24
4	A34	C13	E21	C41	E45	D31	F21	D25	E34	F14	C54	E52
5	D34	C24	F21	F13	E51	B31	D32	E42	F12	D14	C32	A35
6	A12	D13	B32	C24	F12	B42	A32	B31	F23	D53	B32	F13
7	D51	E52	B54	E24	A51	F53	A15	E25	D45	C31	A15	F35
8	F23	D52	C15	D41	B45	E25	B15	F31	A34	E31	F54	C41
9	C23	E41	A54	A52	B23	D25	A21	C25	D12	B42	D43	C35
10	C12	A41	B15	E41	B34	F42	C54	D42	B51	C25	E43	F41
11	C51	F35	D32	E13	C45	A42	C43	B53	E45	E53	D54	F24
12	A45	F41	D21	B35	C12	F25	B54	D31	F34	A31	D32	E24
13	C34	D35	E32	F41	C23	D53	F15	A53	B23	B53	A54	F52
14	E23	F52	A43	E35	D45	C42	A43	C31	F45	B14	C15	E13
15	A51	B13	C54	C35	A12	E14	E21	C14	B12	E14	B15	C24
16	E31	F24	B21	A35	F34	E53	C15	D53	A51	A25	F21	C13
17	F45	C35	B43	B24	A34	C53	B21	E31	C51	D42	C21	B35
18	D23	A52	F43	D13	F45	A14	F43	E53	C45	F25	E15	D13
19	D12	E24	C21	C13	D23	F31	E15	C53	F51	E42	A43	D24
20	D45	B24	A21	E52	D34	B14	C32	F53	A12	E25	A32	B13
21	A23	E13	F54	D52	C51	E31	E43	D14	A23	A42	D21	C52
22	F34	A35	E43	A24	D12	F14	C21	F25	E51	C14	E54	B52
23	E34	B35	D15	D35	F51	C31	F54	B25	D51	F53	B54	A52
24	C45	B41	F32	F52	E23	A31	D15	F14	B45	D25	F32	E41
25	B23	C41	A32	D24	A23	B53	E54	B14	A45	C53	B43	D41
26	F51	D24	A15	F24	A45	D42	D54	A31	E23	B31	E21	A13
27	B34	D41	E15	B41	F23	E42	A54	E14	B34	B25	F43	D52
28	B45	A24	D54	B13	E34	D14	B43	A14	E12	D31	E32	A41
29	B51	E35	F15	A13	E12	C14	B32	C42	D34	A14	B21	E35
30	B12	F13	C43	B52	D51	A25	E32	F42	D23	C42	A21	D35

I

Analysis of Scores

For each of the four batches of children, we estimated the main effect of, for example, type A questions by

$$a = \mathrm{A} - \frac{\alpha}{3}$$

where A $=$ sum of scores for type A questions,

α $=$ sum of scores of subjects doing type A questions;

then $\Sigma a = a + b + c + d + e + f = 0$.

We estimated also a correction for set effect, for boys and girls separately, by, for example,

$$a_1 = \frac{1}{15}\,\Sigma(a_1 - a_k)$$

where A1AK, or AKA1, are a pair of sets done by the same subject, summed over all such pairs including A1;

$$\text{then } \mathop{\Sigma}_{i} a_i = 0$$

The effect of the set A1 was then taken as $a + a_1$, and similarly for the other sets.

The score effect s_i, for the i^{th} subject, was obtained for each batch separately from

$$s_i = \frac{1}{6}\,S_i - \frac{1}{180}\,G - \frac{1}{6}\,\text{(sum of effects of sets done by this subject)}$$

where S_i is the subject's observed score and G is the total score of subjects in that batch. (s_i is thus an estimate of the subject's score on an average set of 18 questions).

The difficulty of the k^{th} question in the set A1 was estimated for boys and girls separately by

$$q_k = \frac{1}{12}\,Q_k - \frac{T_1}{12 \times 18} + \frac{a_1}{18}$$

where Q_k is the total number of right answers to the k^{th} question (from 12 children) and T_1 is the total number of correct answers to the 18 questions in this set.

Then $\Sigma q_k = 0$ summing over questions of the same type (actually, we worked with $12q_k$).

II

ANALYSIS OF TIMES

The times were read from the recording rolls by a scale which transformed them to logarithms to the base $10^{\frac{1}{2}}$; there was also an arbitrary scaling factor of about $\frac{3}{35}$, so that what we shall henceforth call a time is in fact 2 $\log_{10}$ (time in seconds) $- 2{\cdot}1$.

Since the experiment could not be balanced with respect to the appearance of right and wrong answers, the times were first examined to see whether a right answer took a longer or shorter time than a wrong answer. For each set of 18 questions, which were done by six subjects from each batch, we fitted a constant γ by taking the maximum likelihood solution of

$$t_{ij} \doteq \mu + \alpha_i + \beta_j + \gamma + \epsilon_{ij}$$

where t_{ij} is the time spent by the i^{ht} subject on the j^{th} question, and γ is omitted if the question is answered incorrectly, so that γ is a measure of the extra time taken to get a question right.

$$\text{then } \hat{\gamma} = \frac{T_r - \dfrac{\Sigma N_i T_i}{18} - \dfrac{\Sigma N_j' T_j'}{6} + \dfrac{NT}{6 \times 18}}{N - \dfrac{\Sigma N_i^2}{18} - \dfrac{\Sigma N_j'^2}{6} + \dfrac{N^2}{6 \times 18}}$$

where N = number of correct answers,
T_r = total time of correct answers,
T_i = total time for i^{th} subject,
T_j' = total time spent on j^{th} question,
N_i = total score of i^{th} subject.
N_j' = number of times j^{th} question was answered correctly.

There were 20 such $\hat{\gamma}$'s for each type of question, one from each of four batches and each of five sets, so that altogether there were 120. We tested eight of these by using

$$\text{var } \gamma = \frac{\text{residual mean square}}{N - \dfrac{\Sigma N_i^2}{18} - \dfrac{\Sigma N_j'^2}{6} + \dfrac{N^2}{6 \times 18}}$$

and found one significant at the 1 per cent. level (this was in fact the $\hat{\gamma}$ with the highest value), one significant at the 5 per cent. level and the rest not significant. There was, however, no consistent arrangement of the values obtained; we had expected, for instance, that sets of the same type and done by subjects of the same batch would yield similar results, but nothing of this sort was detected either by inspection or by analysis of variance. The average value of $\hat{\gamma}$ for the whole set of 120 was $-\cdot 019$, and the standard deviation calculated from this set was ·13 (which was very similar to the standard deviations calculated in the eight individual cases mentioned above). We therefore decided that the effect, if it existed, was not of sufficient importance in this case for us to need to make allowance for it. It should be noted, however, that the three types for which $\hat{\gamma}$ was on the average greater than 0 were those in which a choice of answers were offered.

The rest of the analysis of the times was the same as that for the scores; an estimate t_i of the time spent by the i^{th} subject on an average set of 18 questions corresponded to the previous s_i, and an estimate l_k of the length of time spent on the k^{th} question by an average subject corresponded to q_k. The relation of s_i to t_i, of s_i to the score on the intelligence test, AH 4, and of q_k and l_k were shown on scatter diagrams.

Estimates of the effect of position in the series of sets of questions, of sex, age, type of question, or any interaction of these were made by adding the relevant observed scores or times, since the balance of the experiment allowed this.

TABLE I

Relationship of Difficulty of Question and Time Spent on it

Type		*Difficult*	*Easy*	$\sqrt{\frac{\chi^2}{N}}$
A	Slow	69	24	·37
	Fast	33	54	
B	Slow	62	34	·12
	Fast	44	40	
C	Slow	79	19	·47
	Fast	28	54	
D	Slow	58	39	·15
	Fast	37	46	
E	Slow	52	44	·05
	Fast	41	43	
F	Slow	44	40	·04
	Fast	46	50	

TABLE II

Total Scores

	Position						*Total*
	1	2	3	4	5	6	
14-year-old Boys	188	200	181	181	175	170	1095
13 ,, ,, ,, ...	170	191	171	170	178	160	1040
14 ,, ,, Girls	175	167	145	153	165	174	979
13 ,, ,, ,, ...	129	125	139	124	140	129	786
Total ..	662	683	636	628	658	633	3900

ANALYSIS OF VARIANCE

	s.s.	*d.f.*	*m.s.*	*Var. ratio*
Positions	566·5	5	113·3	Not sig.
Sex	5704·2	1	5704·2	58·09 } sig. at ·1% level
Age	2562·7	1	2562·7	26·10 }
Sex x Age	793·5	1	793·5	8·08 sig. at 5% level
Residual	1473·1	15	98·2	
Total ..	11100·0	23		

TABLE III

TOTAL TIME MEASURES

	Position						*Total*
	1	2	3	4	5	6	
14-year-old Boys ..	811·5	674·1	681·2	577·3	656·4	549·6	3950·1
13 ,, ,, ,, ..	833·7	685·5	692·6	568·0	682·1	579·6	4041·5
14 ,, ,, Girls ..	805·3	670·7	649·2	508·6	629·7	551·9	3815·4
13 ,, ,, ,, ..	803·3	677·1	635·8	542·2	639·7	597·0	3895·1
	3253·8	2707·4	2658·8	2196·1	2607·9	2278·1	15702·1

ANALYSIS OF VARIANCE

	s.s.	*d.f.*	*m.s.*	*Var. ratio*
Positions	176881·62	5	35376·32	
Regression	113572	1	113572	393 } sig. at ·1% level
Deviation from regression	63310	4	15828	55 }
Sex	3292·38	1	3292·38	10·3 sig. at 1% level
Age	1219·80	1	1219·80	Not sig.
Sex x Age	5·70	1	5·70	,, ,,
Residual	4290·07	15	286·00	
Total ..	185689·58	23		

Regression slope, $b = -40·3$.

MISCELLANEA

A NOTE ON THE PAIRED COMPARISONS METHOD OF RATING

BY

NANCY HARRIS

(*Medical Research Council Applied Psychology Research Unit, Cambridge*)

In psychological work where assessment of the ability of a number of individuals is required, paired comparisons are often preferable to any other method of rating. But if this method is used, the number of subjects rated in any one group must be kept small; 20 subjects, for example, will involve the rater in 190 comparisons.

When a large group of subjects is to be assessed, it is usually possible to sub-divide it for purposes of rating. But the nature of the division is often dictated by circumstances outside the investigator's control, and it is common to find that the resulting sub-groups are of unequal size. The data from a number of unequal sub-groups may usually be analysed separately, and the results summated to give a figure for the whole group. χ^2 and $\hat{\iota}$, for example, are appropriate statistics to use in this way.

Sometimes, however, a single set of ratings on the whole group of subjects is exacted by the aims of the research. The following method of recombining the paired comparisons data from a number of sub-groups was found useful in a recent investigation where this situation arose.

Suppose that a group of 30 subjects are to be assessed on proficiency in a certain job. The available raters are such that the group can be sub-divided in one way only—that is into four sub-groups numbering, respectively, 6, 6, 9, 9. Each of these groups can be assessed by three independent raters. For each group, the number of times each subject is judged the better of a pair is counted, and preference scores are obtained in this way for every subject. Now if the raters were all perfectly self-consistent and if the three raters for each group were in perfect agreement with each other, we should obtain the following sets of total preference scores:

Subject:	*A*	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*J*
Sub-groups I and II (6 subjects in each)	15	12	9	6	3	0			
Sub-groups III and IV (9 subjects in each)	24	21	18	15	12	9	6	3	0

In fact we find that every rater is highly self-consistent but not perfectly so, and the agreement between the three raters for each sub-group is, in every case, significant but not perfect. The observed preference scores for the four sub-groups are as follows:

Subject:	*A*	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*J*
Sub-group I	15	10	8	8	4	0			
,, ,, II	13	12	11	4	3	2			
,, ,, III	24	21	17	14	12	10	7	3	0
,, ,, IV	22	20	20	16	12	8	5	4	1

It is possible to match these observed preference scores with the theoretically perfect ones. We can take the smaller sub-groups (I and II) as defining the number of grades of differentiation and regard the perfect preference scores as the boundaries of five grades as follows:

Grade 1	12–15
Grade 2	9–12
Grade 3	6–9
Grade 4	3–6
Grade 5	0–3

We can fit the sub-groups I and II into these grades directly, adopting the convention of putting scores which coincide with a boundary figure into the grade nearer the middle. For the larger sub-groups, III and IV, a conversion must be made. The maximum theoretical preference score for these groups is 24, and by dividing this by 5 the size of the grades and the boundary figures are determined. They would be:

Grade 1	19·2–24·0
,, 2	14·4–19·2
,, 3	9·6–14·4
,, 4	4·8– 9·6
,, 5	0·0– 4·8

The following classification of the observed data can then be made:

Sub-group:	*I*	*II*	*III*	*IV*
Grade 1 . .	A	A	A, B	A, B, C
,, 2 . .	B	B, C	C	D
,, 3 . .	C, D	—	D, E, F	E
,, 4 . .	E	D, E	G	F, G
,, 5 . .	F	F	H, J	H, J

As the theoretically perfect preference scores always follow a regular series, this procedure can be used to classify the whole group into any number of grades, up to the point when the grading approximates to a ranking.

The method described enables data from sub-groups of different sizes to be combined, and it preserves differences of distribution within the groups. The main criticism is that it assumes every sub-group to be of similar overall standard. When all the sub-groups are drawn from the same source it may be possible to arrange that this is true; failing that, it may be possible to select members from each sub-group and by having them assessed in a further mixed group to check up on the relative standards of the sub-groups. But when the sub-groups come from different sources there is no way of estimating how far they are of a common standard. This criticism would, of course, apply to data obtained by any method of rating and combined in any way. By using this method of treating paired comparisons one can at least be certain that the average level of classification grade 1 is higher than that of grade 2, and so on, even if the extent of variation *within* any one grade is unknown.

BOOK REVIEW

The Perception of the Visual World. By J. J. GIBSON. Boston: Houghton Mifflin. 1950. Pp. xii + 235.

In his introduction to this book, Professor Carmichael says that "the study of perceptual problems is central in psychological theory." The reviewer is in complete agreement with that statement; and there can be no doubt of the value of Professor Gibson's contribution to this study. One may not be able to agree completely with his argument—and indeed its exposition is not always as straightforward as might be wished. But it is certain that everyone who is interested in this central problem of psychological theory should study that argument.

Professor Gibson presents a theory of perception which endeavours to resolve the old empiricist-nativist controversy, and to reconcile the differences between the Gestalt psychologists and those who hold that the primary sensory data must be developed and interpreted in the light of experience before meaningful percepts are obtained. He is also concerned to controvert recent theorists who have maintained the view that perception is determined to a considerable extent by individual personality factors, and that the perceived world is an individual construct. This, he says, cannot be so, since the visual world differs comparatively little between different observers, and agrees remarkably well with what is actually known as to the nature of the environment. "The properties of the literal visual world, in so far as conditions are optimal, approach an exact correspondence with variables of physical stimulation." But this "visual world" must be distinguished from the "visual field"—what can be observed in introspective analysis at any one moment by the single stationary eye, and what has sometimes been thought of as the primary sensory impression. The visual field consists of flat patches of colour, bounded by the periphery of vision; it corresponds fairly closely with the pattern of retinal excitation, though the latter is a correlate, not a picture, of the external field. But the visual world is continuous, unbounded, possessing depth and solidity; it is built up from the primary memory images of successive and overlapping retinal impressions, obtained by changes of fixation which are so frequent and so randomly directed that no awareness of succession remains.

It is clear that evidence of the truth of this theory must be based upon experimental observation of the manner in which varying impressions of the visual field are built up from varying visual fields. This Gibson proceeds to do by considering the phenomena of form and of the arrangement of objects in space. He states that the Gestalt laws of organization of form are on the whole misleading because they were obtained from the study of isolated and often ambiguous two-dimensional shapes. Re-organization of the field in accordance with the Gestalt laws does not occur with ordinary solid objects distributed in space; their shapes are clear, unambiguous and rigid. The figure-ground experience is also quite unambiguous, being given by the edges of these objects perceived from simultaneous sudden changes in gradients of colour, brightness, texture and distance.

The apparent stability of the visual world and of objects in space is explained as follows: When the eye or the head move, the order of arrangement of excitations is maintained temporarily and spatially; and the movement of this orderly pattern across the retina is compensated by kinaesthetic sensations of movement of the eyes and head. Thus the world does not appear to move or change. In the perception of real movement, there is again an orderly succession of changing retinal impressions, but the kinaesthetic sensations do not compensate exactly.

The perception of distance Gibson attributes to the occurrence of a regular serial order of surfaces along the ground, graded from near to far in texture, size, slant of shape (perspective), hue, clarity, disparity and deformation with movement (parallax). Gibson's work on the varying deformation of the perceived landscape with aircraft movement has shown how valuable this phenomenon can be in the estimation of distance. When no perception of receding surfaces is possible, localization is difficult if not impossible. (This is demonstrated by a neat experiment described later in the book in which it is shown that judgment of size depends on the apparent distance as given by the part of a receding surface with which the test object is in contact.) But in contradiction to Gibson's theory, there is much evidence to show that relative localization at least is possible with disparity functioning in the absence of other perceptual data. The impression of distance is based, not on the accurate gradation of disparity from near to far, but on the correspondence of the retinal images of points equidistant with the fixation point, and non-correspondence of the images of other points.

The distinction between the visual field and the visual world seems at first to come in appositely when we consider the variations of colour, shape and size constancy which have been observed to occur when there is a change from the "object" to the "stimulus" attitude. The latter of course gives the observer the analytical impression of the visual field. With the former, he observes the visual world, and his shape perception is integrated with his perceptions of the orderly distortion of outline and surface texture with tilt of the surface. But Stavrianos has shown that shape constancy is maintained when judgment of tilt is quite inaccurate. And recently Langdon, at Oxford, has gone further and shown that it is independent of texture or brightness differences. It must be noted that Gibson is somewhat too much inclined to present *prima facie* pronouncements as to the importance of factors such as surface texture without adequate experimental evidence of their actual functions.

Size constancy Gibson attributes to the fact that the size of any given object is given by the scale of the background, in the total visual world, at the point to which the object is attached. Thus the results of Joynson's experiments, showing that constancy may be impaired by observing the visual angle covered by the object, would presumably be due to the fact that the attachment is to a narrow segment only of the momentary visual field. Some objection must be made to Gibson's contention that the size constancy of objects holds as long as they can be seen at all. Any one who has sat on the side of a mountain and watched beetle-like cars and ant-like men crawling along a road in the valley will agree how genuinely minute they appear. Or is this attributable to the lack of gradually receding surfaces between the observer and the objects?

Finally, it is clear that the visual world possesses another characteristic as basic and fundamental as those of form and spatial arrangement—that of meaning. It seems probable that all perception, from birth onwards, has meaning of a kind, though the meanings attributed by the infant to his percepts are very different from those utilized by the adult, which are schematically determined from what he learns about the environment. Gibson has less to say on the subject of meaning. But there seems no sort of reason why he should postulate that the existence of such schematically determined meanings is incompatible with accurate perception of what he calls the "literal world," the properties of which "approach an exact correspondence with variables of physical stimulation." From his earliest years the child learns that in order to come to terms with his physical and social environment, he must perceive at least certain aspects of the world with sufficient accuracy to constitute a basis for understanding their effective meaning. The normal type of percept is then a construct in which are integrated impressions (colour, shape, etc.) correlated with variations in the physical variables, and meanings for the most part laid down schematically through active "trial and check" and social training. Individual meanings are found to occur when the physical conditions are inadequate to provide clear and unambiguous impressions. Then, as Bruner has pointed out, the individual is bound to make hypotheses which will be based on individual knowledge and interest in so far as the habitual schemata which he possesses in common with others are inadequate to provide meaning. Any acceptable theory of perception must account for the discrepancies as well as for the agreement of perception between different individuals. M. D. V.

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A STUDY OF POSITION HABITS INDUCED BY REWARD AND "FRUSTRATION"*

BY

ROGER W. RUSSELL† and ROBERT G. F. PRETTY

(*From the Department of Psychology, Institute of Psychiatry* (*Maudsley Hospital*), *London*)

Predictions from Maier's theory of "frustration"-instigated behaviour have been tested in an experimental situation differing significantly from that in which the theory was propounded yet containing the central element of "frustration"—the insoluble problem. A water discrimination unit was employed in which the performance of rats would be observed during attacks on insoluble problems, position problems or symbol problems.

Two groups, each containing ten Wistar albino rats, served as subjects. The research design consisted of the following phases: preliminary training, development of position responses, exposure to a symbol-reward problem with 50 per cent. punishment and exposure to a symbol-reward problem with 100 per cent. punishment. The design differed for the two groups only at the phase in which the position responses were established. During this phase one group was exposed to a position-reward problem and the other to an insoluble problem.

Position responses were established as frequently under position-"frustration" (position stereotypes) as under position-reward (position habits) conditions. Position stereotypes were more rigid—more resistant to extinction—than position habits under conditions of 50 per cent. punishment. Position stereotypes were as readily extinguished under 100 per cent. punishment as were position habits under 50 per cent. punishment.

The first two observations conform to predictions made from Maier's theory. The third does not. That is to say, not all situations containing the basic elements of "frustration" give rise to stereotyped behaviour patterns which are as rigid or "fixated" as Maier's theory would predict. It is a reasonable hypothesis that the characteristics of stereotyped responses established in certain "frustration" situations may be described adequately in terms of conventional learning principles without the necessity of resorting to a distinction between "goal-motivated" and "frustration-instigated" behaviour.

I

INTRODUCTION

THE extensive work of Maier and his co-workers, summarized in his recent text on "Frustration: The Study of Behaviour Without a Goal" (1949), has important implications concerning the aetiology and nature of "abnormal" behaviour patterns. They have found that stereotyped behaviour patterns develop when subjects (rats) are forced to respond in an insoluble problem ("frustration") situation. Objectively observed, these behaviour patterns have much in common with position habits established under conditions of reward reinforcement. However, such stereotyped responses may differ very significantly from position habits in terms of their persistence when a soluble (symbol reward) problem is introduced after the stereotyped

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† Fulbright Advanced Research Scholar and Director, Animal Psychology Laboratory.

response has been established. Stereotyped responses may show much greater resistance to extinction than do position habits. As a result of these observations, Maier proposes that "frustration-instigated" behaviour differs qualitatively from "goal-motivated" behaviour and, therefore, is describable in terms of different principles.

The present paper reports the first in a series of experiments designed to study the implications of Maier's theory. The majority of experimental data upon which this theory is based have been obtained by controlled observations of the rat's behaviour in the Lashley Jumping Apparatus. Systematic variations of environmental conditions have been introduced within the limits of this experimental situation. However, it seems logical that, if Maier's conclusions have the broad significance he imparts to them, similar results would be predicted in other experimental situations containing the elements of "frustration" as Maier has defined them. The present research has employed an experimental situation which differs from Maier's in the kind of motivation used, the type of response studied and the nature of the reinforcement employed, while retaining the "frustration" of the insoluble-problem situation. The results support predictions from Maier's theory regarding the development of stereotyped position responses, but show certain divergences from predictions concerning the rigidity of the stereotyped response once established.

II

RESEARCH DESIGN AND PROCEDURE

A. Research Design

The research design employed in the present study is presented in Table I. Subjects were selected at random from the highly inbred laboratory population and throughout the research were treated identically except for the differences required by the research design.

TABLE I

RESEARCH DESIGN

Group	*N*	*Phase*			
		1	2	3	4
Position-"frustration"	10	Preliminary training	Insoluble problem	Symbol problem: 50% punishment	Symbol problem: 100% punishment
Position-reward ..	10	Preliminary training	Position problem	Symbol problem: 50% punishment	Symbol problem: 100% punishment

This design allowed the testing of three predictions derived from Maier's theory. First, position responses, overtly alike in character, would develop at phase 2 in both experimental groups. Second, at phase 3 the position responses would undergo rapid extinction and symbol responses would be readily established in subjects of the position-reward group, but not in subjects of the position-"frustration" group. And, third, a shift to 100 per cent. punishment in phase 4 would not alter the position responses of the position-"frustration" animals.

B. *Apparatus*

The apparatus employed in presenting the soluble and insoluble problems was the water discrimination unit described by Russell, Jonckheere and Williams (1950). The animals were required to swim the length of a straightway in order to reach two doorways through which they could escape from the water. Each doorway contained a panel on which was painted a symbol. Below this panel was a door hinged in such a fashion that it could easily be pushed open by the animal. The symbols—black circle on white background or white circle on black—could be moved from one doorway to the other, and each door could be locked independently of the symbol above it. This made it possible to present to the animal any one of the problem situations required by the research design. A position problem could be presented by always locking the door on one side regardless of the symbol above it; a symbol problem, by always locking the door beneath the same symbol; and an insoluble problem, by shifting the symbols and locking the doors in independent random sequences. Motivation was controlled by maintaining the temperature of the water at 18° ± 1° centigrade. A response to the unlocked door was reinforced by immediate escape from the water, while a response to the locked door was punished by delaying escape until the animal swam around a partition between the doorways and pushed through the unlocked door. The difference between reward and punishment, therefore, was defined in terms of the duration of time in the water.

C. *Subjects*

Twenty male albino rats of the Wistar strain were used as subjects. One position-reward animal was rejected after phase 2 because of illness. All animals were approximately 230 days of age at the beginning of the experiment. During the hours between experimental trials they were kept in normal living cages in the animal room and maintained *ad libitum* on MRC diet 41A plus water.

D. *Procedure*

The procedures followed in each of the four phases of the research are described below.

1. *Phase 1: Preliminary Training.* Preliminary training was carried out in three stages. No symbols were presented during this period. All animals were given six trials per day for six days. Both doors were left open on the first three days, the animal being required to swim the length of the straightway and climb through a doorway to reach the escape platform. On the fourth and fifth days one of the doors was propped half open and released when the animals started to climb through; the other door was locked. Both doors were closed during the sixth day and the animals required to push the unlocked door open in order to escape. Preliminary studies had shown that such a training period was sufficient to orient the animal to the discrimination end of the water unit and to teach it the procedure for opening the doors.

2. *Phase 2: Position problem.* Twenty-four hours after the position-reward animals had completed their preliminary training they were placed in the water unit for their first learning trial. Ten trials were given per day until the animals had reached the criterion of 155 responses to the same position in the last 160 trials. This criterion was selected to correspond to the criterion employed by Maier and his co-workers in their research on "frustration." The unlocked door was always the one chosen by an animal on its first trial, thus corresponding to any position preference the animal may have had. The symbols were shifted at random but the locked door was always in the same position.

3. *Phase 2: Insoluble problem.* This same general procedure was followed with the position-"frustration" group except that the symbols were shifted and the doors locked in independent random sequences. Under these conditions the majority of animals developed position stereotypes to the criterion before 400 trials had been given. Those who did not reach this criterion were advanced to phase 3 of the research design at the end of the four-hundredth trial.

4. *Phase 3: Symbol problem, 50 per cent. punishment.* After completing phase 2 all animals were presented with a symbol problem in which the door below the black circle on the white ground was always unlocked regardless of the position in which it appeared. The symbols were shifted in a random sequence. Thus an animal who persisted in its position response was punished on 50 per cent. of its daily trials. Under these conditions all the position-reward animals and one of the position-"frustration" group reached the criterion of seven successive correct responses in one day's "runs" within 300 trials.

5. *Phase 4: Symbol problem, 100 per cent. punishment.* Those animals in the position-"frustration" group who did not solve the symbol problem with 50 per cent. punishment within 300 trials were advanced to phase 4 of the research design. In this phase an animal who continued to make position responses was punished on 100 per cent. of its daily trials. When the original position stereotype was broken the animal was rewarded for five consecutive responses to the opposite door. From then on any position response was punished on 100 per cent. of its trials since the objective of this phase of the research was to determine whether or not position stereotypes established in earlier training could be broken by repeated negative reinforcement.

III

RESULTS

The results are summarized in Tables II and III. It will be noted that all the position-reward animals developed position habits within the 400-trial limit set by the research design. In all cases these habits were extinguished and symbol habits established within the 300-trial limit set for phase 3 of the procedure.

TABLE II

RESULTS FOR THE POSITION-REWARD GROUP

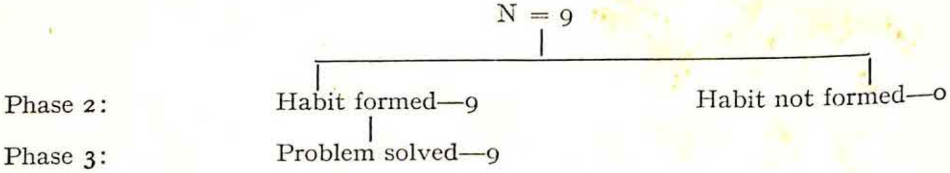
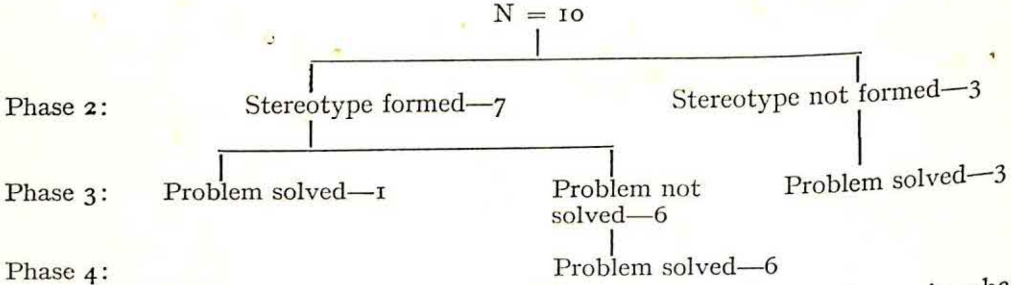


TABLE III

RESULTS FOR THE POSITION-"FRUSTRATION" GROUP



Seven of the position-"frustration" animals developed stereotypes in phase 2, while three did not reach the criterion. All the latter established symbol habits in phase 3 and only one of the former. All six of the animals not developing symbol habits in phase 3 did establish them in phase 4.

Yates' exact test for 2 × 2 tables (Fisher, 1948) shows no significant differences between the two groups in phase 2, indicating that position responses were established as frequently under position-"frustration" as under position-reward conditions. On the other hand, a similar test of the data for phase 3 does show the existence of significant differences at better than the ·001 level of confidence. This may be interpreted as indicating that position stereotypes are more rigid—more resistant to extinction—than position habits, in which case the results support predictions from Maier's theory.

In phase 4 the punishment was increased to 100 per cent. Under these conditions all the six animals who had shown resistance to extinction in phase 3 did break their position stereotypes and developed symbol habits. This observation does not correspond with predictions from Maier's theory.

It is interesting to compare the number of trials taken by the two groups in extinguishing their position responses and developing symbol habits. The position-reward group took 138·33 (S.D. 72.58) trials under conditions of 50 per cent. punishment to reach the criterion for the symbol habit. The one animal of the position-"frustration" group establishing a symbol habit under these conditions of reinforcement reached the criterion in 202 trials, ·87 standard deviations above the mean of the position-reward group. The six animals developing symbol habits under 100 per cent. punishment took 99·17 (S.D. 31.53) trials to reach the criterion.

IV

DISCUSSION

The results of this experiment show that position responses may be established under conditions of "frustration" as well as under conditions of escape training. However, the more interesting observations are those indicating a differential reaction of position responses established under these two sets of conditions to extinction during the development of a new response pattern.

Maier has suggested that this differential reaction reflects qualitative differences between "goal-motivated" and "frustration-instigated" behaviour patterns: ". . . abnormal behaviour fixations produced under frustration are different in kind from behaviours produced through motivated learning." Maier's results go further to suggest that these qualitative differences may reflect a differential effect of the conditions under which the position response is established on the *performance* of the response as compared to the *learning* of a new response. This is based upon the distinction between performance and learning made by Tolman (1932) in his learning theory and is supported by Maier's observations that, even though the position response appears, the animal's behaviour during a trial may give evidence that it is in fact able to differentiate the symbols presented.

Examination of the facts in the present experiment suggests two alternative hypotheses, either of which might account for the results obtained and both of which can be tested. One of these hypotheses involves the differential effects on extinction of practice under conditions of continuous and partial reinforcement. Jenkins and Stanley (1950) in their recent review of research findings on this subject present extensive evidence of significantly slower extinction following the development and maintenance of behaviour patterns with massed training under conditions of partial reinforcement. The development of position responses under conditions of the present research design is associated with almost continuous reinforcement for the position-reward group because of the small number of trials to establish the response in relation to the number of trials required by the criterion for establishment. On the other hand, the development of the position response in the position-"frustration" group occurs under conditions of partial reinforcement. In the present experiment, the actual per cent. of rewarded trials for the position-reward group was 98 per cent., and for the position-"frustration" group, only 49 per cent. On the basis of previous studies, it might be predicted that extinction would be slower in the latter group. This may have been the basis for the significantly different performance of the two groups in phase 3.

The second hypothesis arises from an examination of the pattern of reinforcement from the subject's point of view. In the case of the position-reward group a position response led to almost 100 per cent. reinforcement during the development of the position response. When the criterion of establishment had been reached and the experimenter had altered the nature of the problem, a position response led only to 50 per cent. reinforcement. The pattern of reward and punishment had been altered significantly and, as the results indicate, the behaviour changed in relatively few trials—138·33. For the position-"frustration" group a position response led, during the development of the stereotyped response, to only 50 per cent. reinforcement. After the criterion of establishment had been reached and the problem altered from the experimenter's point of view, a position response still led to 50 per cent. reinforcement. The pattern of reward and punishment had not changed as far as the subject was concerned. As soon as this pattern was changed by imposing 100 per cent. punishment for position responses the position-"frustration" animals established the symbol habit in 99·17 trials. The possibility exists, therefore, that the initial position response was altered in one case as the results of a perceptible change in the pattern of reward and punishment, whereas no alteration of response occurred in the other case because no such change was perceived.

Neither of these hypotheses appears to be applicable to Maier's results, since many of his animals persisted in their position responses even after as many as 600 symbol-reward trials and after variation in the per cent. of reinforcement.

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THE PERCEPTION OF A CHANGING SHAPE*

BY

J. LANGDON

(*From the Institute of Experimental Psychology, University of Oxford*)

Under specified conditions a pair of simple shapes are matched by a subject when almost all supplementary textural and space cues of depth vision have been removed. Under such conditions it is found that shape constancy is no longer present. However, the effect of regular rotatory motion of the shape is sufficient to restore constancy in the continued absence of other cues. Degree of perceptual constancy appears to be correlated with rate of change of shape. It is suggested that an explanation of the phenomenon is to be sought along the lines of Michotte's concept of "object creation" rather than in terms of gradient variables.

I

INTRODUCTION

THE present paper has been given this very general title but does not pretend to discuss the subject exhaustively. These observations are merely part of a more general investigation of shape perception at present being carried on by the writer, and it is hoped that it may be possible to publish a paper dealing with other aspects of this extensive subject.

Over a number of years many investigations of the appearance of a shape under various conditions have been made. In nearly all these studies the degree of perceptual constancy has in one way or another been made the measure of the "goodness" of the perception. That is to say, the degree to which the perception and the behaviour based upon it are more stable than their immediate objective conditions.

The earliest investigators presented alternately two shapes in a darkened room (Götz Martius, 1889) whilst members of the Gestalt School (Eissler, 1933; Klimpfinger, 1933) used shapes presented simultaneously, but separated by a large included angle. In the latter types of experiment comparison was made between one shape which is rotated about an axis and another which remains stationary. The shapes employed were usually circles, ellipses or squares such that the comparison could be approximated to the standard at some projective angle. During the actual rotation of the shape the subject is unable to see the objects, these being temporarily screened off. Stavrianos (1945) used a more elaborate technique in which these type comparisons were correlated with others in which the subjects had to estimate the real angle of orientation.

Thouless (1931) used a different method of comparison, his procedure being to present one shape as distant and foreshortened and the other as a projection of this, seen frontal-parallel and near. The near shape matched with the far was selected from a family of similar projections. For a full discussion of the history of experiments on shape perception the reader should consult the papers of Brunswik (1933), and Joynson (1949), on the problem of definition of shape. A general theoretical discussion of the problem can be found in Vernon (1937), Gibson (1950) and Koffka (1935).

From the standpoint of the present investigation all the various studies can be subsumed under a single heading, despite their theoretical disagreements, in that all are, in essence, investigations of the appearance of *stationary* shapes. Whether

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this is carried out by alternate inspection of two shapes or by simultaneous presentation, whether the comparison is made by rotating a shape to a judged match or by choice of a member of a family of shapes, in all cases the subject has before him a stationary shape which he may inspect for a relatively unlimited time. In addition to this, he is not able actually to see any process of change in the shape. Whether he has knowledge of how this takes place, or has any knowledge of the true character of the objects before him, and, if he has such knowledge, whether or not it influences his perception in the final outcome, might be speculated upon at some length (cf. "La Developpement de la Perception Visuelle," *Arch. Psychol.*, **93**, 1941).

What is clear is that the observer has no direct *perceptually given* knowledge as to this process of change. All that is before him is a certain fixed situation which he inspects and then, before the situation is changed for another one, makes such comparisons as appear to him reasonable. It is germane to the discussion to point out that in a number of experiments the results obtained when a subject was allowed to inspect and examine the set-up failed to show any significant differences from those in which judgments were made in the absence of such acquaintance. It would seem that intellectual, acquired knowledge does not influence perception in any directly measurable way. On the other hand knowledge of the situation acquired in the form of practice would appear to influence perception (cf. Klimpfinger, 1933).

At the same time there is definite evidence that variation in the rate at which a series of perceptual objects is presented can affect the outcome of the experiment. Thus Asch and Witkin (1948) reported that in experiments where a subject had to orientate a self-luminous rod to a judged vertical and/or horizontal setting, the presence of the "A" (Aubert) or "E" phenomenon could be noted under various different conditions, the rod being moved sometimes continuously and at others in a series of jumps. Under this last condition the amount of "oversetting" could be influenced by variation of the rate at which these jumps occurred. (The "A" and "E" phenomena are the errors made in perception of an oriented stimulus object when the subject's body is tilted. The amount and phase of error undergo sharp changes according to the extent of body tilt.)

The present writer has found in a similar way that, if the constancy experiments described above are performed without the interposition of a screen during adjustment of the shape, the constancy may be influenced by the rate at which alterations of shape are produced. And this change may be generalised as being in the direction of lowered constancy with slow or irregular changes and a rise in constancy when changes are regular and frequent.

Eissler and Klimpfinger in their experiments endeavoured to list a number of cues or determinants of shape perception and rate their importance. Among these were binocular vision, reflectance ratio, illumination and texture or microstructure. Apart from these "objective" determinants are also listed subjective or "dynamic" factors such as attitude of the subject ("pole of intention"—Brunswik) and the effect of practice. And it can be shown that the degree of constancy is progressively reduced by the removal of supplementary perceptual cues. But in the case of shapes rotated discontinuously, i.e. the subject being able to see movement taking place, the writer found that removal of the surface of the shape by substitution of a ring for a surface, did not entirely abolish perceptual constancy. Here, as in Boring's experiments on size constancy (Boring, 1941), the law of the visual angle was always exceeded even under the most unfavourable conditions. However, in this type of experiment there remains always one set of cues, namely, the appearance of the room in which the experiment is performed. This contributes the three-dimensional setting, the visual field in which the change of shape takes place. In order to eliminate this last set of cues, the experiments were repeated in a darkened room using whitened wire outline shapes as test objects.

The procedure was to carry out the experiment with the room under normal illumination, the wire outline appearing as an abstract shape on a black ground. After obtaining matches under these conditions the room was then darkened and the shape set at the mean position given previously. The subjects were then asked if they agreed with their past judgments. If they did not (none did), the shape was brought to a new match point. The new judgments all centred about the point required for fulfilment of the law of visual angle. Thus it would seem that in the absence of supplementary cues from the object the normal visual setting had in some way influenced perception.

II

AIM OF EXPERIMENTS

It was decided to investigate the relationship between the perception of a stationary shape and a moving one. In normal and everyday perception objects are seen in movement. This movement is between objects relative to one another and the percipient, or in the case of stationary objects, due to the percipient's own movements. Moreover, ordinary judgments are made with some practical end in view and not as ends in themselves or as part of some scientific experiment. Thus the usual type of shape constancy experiment is, quite legitimately for its purposes, investigating a special class of perceptual phenomena somewhat removed from everyday experience (cf. Brunswik, 1933; Joynson, 1949).

It was thought that there might be some relation between constancy and the perception of movement, or *change* of shape. Gibson (1950) has suggested that two gradient variables for the perception of orientation and shape are the relative steepnesses, or slope, of (*a*) movement across the retina, and (*b*) the skewing of binocularly disparate images. Thus Gibson (p. 172) writes: "The steepness of a gradient towards crossed disparity running from the far to the near margin of a physical surface is proportional to the slant of that surface." If this is the case then it should be possible to isolate such variables and to measure their effect.

III

METHOD

Such an experiment requires that all the determinants to the perception of the real shape of the object must be controlled other than those it is intended to study. Thus all textural features, gradients of illumination, reflectance, etc., must be abolished, together with cues to distance and depth perception emanating from perceived walls, perspectives and so on, of the visual surroundings (Gibson, 1947).

In addition, the stimulus object must be simple and abstract, without internal angles which might assist deductions and help to give rise to analytic judgments based on the perspective of the rotating object itself. These requirements were met by making the object a simple circular outline of wire 1 mm. in thickness and 17 cm. in diameter which rotated on its vertical axis. Together with the circle was prepared a family of ellipse outlines separated by steps equivalent to 5° of rotation of a circle through its arc from 10° to 80°. It will thus be seen that there will be created 15 different ellipses as comparison shapes. The circle and an ellipse were set up 2·5 m. distant from the subject and were separated by a subtended angle of 10°. This included angle is sufficient to prevent the subject fixating both stimuli simultaneously.

The experiment is carried out in a darkroom which has been entirely covered with non-reflecting black paper and has internal traps to catch any stray light. The objects are seen by fluorescence, produced by coating the forms with a suitable substance and exposing them to U.V. radiation. Precautions were taken to avoid corneal fluorescence or any other undesirable effects from the light source. Isolated from the experiment is a control cabin equipped with means of varying the rate of rotation of the shape and noting its angular setting when the subject recorded a match. To indicate a match the subject presses a small push held in the hand. During the experiment the circle rotates from the line of regard through 90° and returns to its starting point, thus providing two matching points, one in each direction, waxing and waning.

The subject is seated comfortably in a chair and views the shapes at eye level, his head resting against the wall to his rear and one eye being covered with a shade. In judging the shapes he is asked not to make head movements. It was considered that any advantages which might accrue from the use of a head rest or biting board would be offset by loss of naturalness and ease for the subject. Subsequent test by the experimenter tended to show that small head movements did not result in any significant difference in scoring.

In order to compare judgments of shapes in motion with judgments of stationary shapes under identical conditions it was considered illegitimate to use shapes "set" behind a screen and then presented for inspection, since in the moving shape situation the subject is able to see all the different aspects of the changing shape and note that it has phenomenal identity and continuity. Therefore another apparatus was constructed whereby the subject was able to adjust the stimulus circle to a point of compared equality with the ellipse by remote control. For this purpose a "Selsyn" device was used so that a small knob could be placed in the subject's hand, and the judgment made and recorded in complete darkness. In this way the identity and continuity of the phenomenal shape was preserved whilst destroying the independence and regularity of the rate of change.

The method of scoring adopted was extremely simple, dispensing with the Thouless or Brunswik formula. Each velocity was tabulated as the only variable, judgments for all the different shapes being aggregated. The measure of constancy was simply the excess angle recorded over and above that required to satisfy the visual angle. Thus for the sum of all shapes, divided by the total number of shapes seen at any one velocity, the constancy value is comparable with similar values at other velocities. In order to measure the dispersions, the S.D. is calculated for each subject and the values averaged for each velocity.

IV

RESULTS

TABLE I

STATIONARY OUTLINE SHAPES IN DARK SPACE: HAND SET

Stimulus shape	35°	40°	45°	50°	55°
Match (in)	37°	41°	46·5°	51°	56°
,, (out)	36°	41°	47°	51°	56°

S.D. for pooled mean (in) = 2·3 Mean constancy (in) C = 0·002

,, ,, ,, ,, (out) = 1·8 ,, ,, (out) C = 0·002

Subjects 12. N = 340.

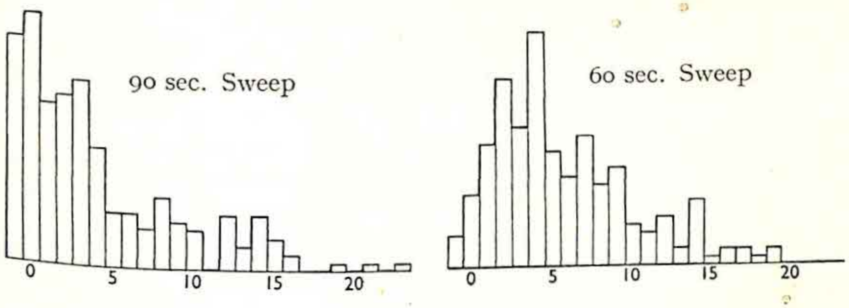
TABLE II

OUTLINE SHAPES IN DARK SPACE: MECHANICALLY MOVED AND MATCHED AT DIFFERENT VELOCITIES. VALUES AGGREGATED FOR ALL SHAPES

Velocity	90 sec.	75 sec.	60 sec.	45 sec.	30 sec.
Mean score	49°	49° 45″	50° 48″	52° 28″	54° 51″
Space error	1° 15″	1° 10″	1°	0° 52″	0° 55″
S.D. (in)	1·4	1·4	1·31	1·66	1·54
S.D. (out)	1·3	1·35	1·35	1·45	1·4
"V" (in)	2·86	2·81	2·58	3·16	2·8
"V" (out)	2·66	2·71	2·66	3·48	2·55
Constancy*	0·09	0·105	0·129	0·167	0·225

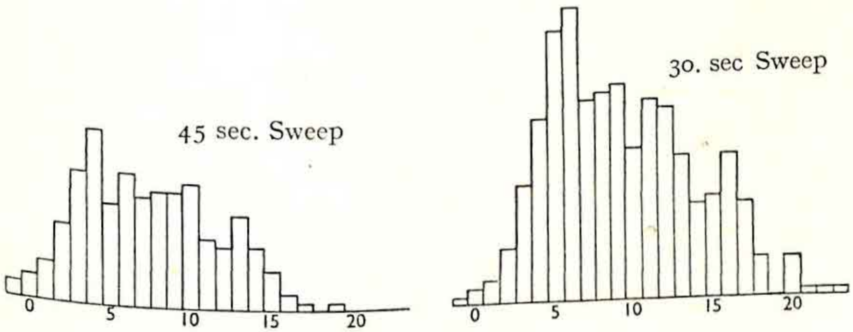
Subjects 12. N = 1140. * Mean Vis. Angle = 45°.

FREQUENCY HISTOGRAMS FOR FOUR VELOCITIES OF SWEEP RATE



Excess Angle.

Excess Angle.

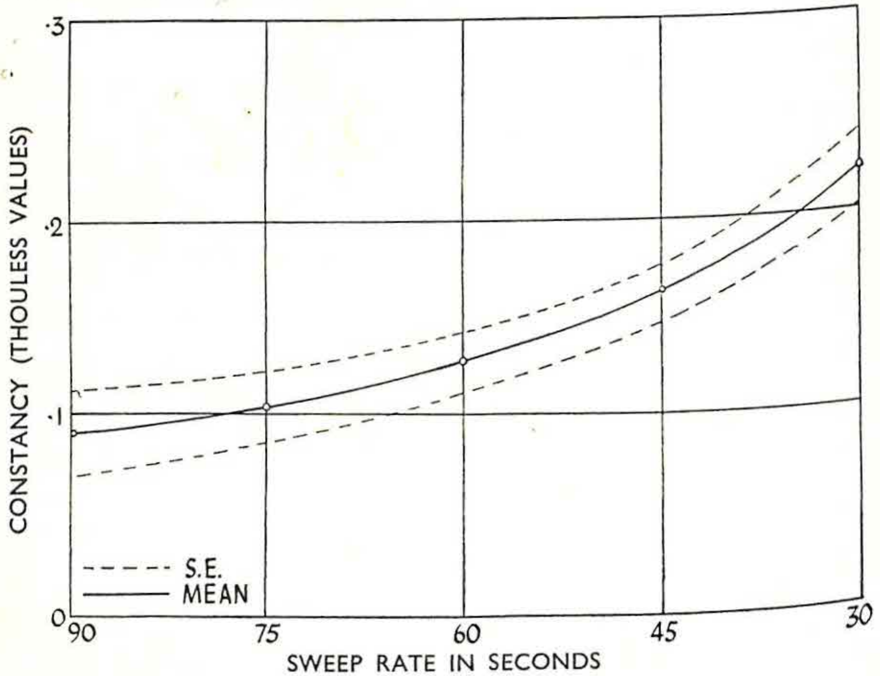


Excess Angle.

Excess Angle.

From a comparison of Tables I and II it may be seen that there is a qualitative difference between the stationary and moving shape situations. Not only is the situation radically different even at the lowest velocity used but the rise in constancy with increase of velocity is continued as a smooth curve. In the case of the stationary, self-adjusted shapes the subject has relatively unlimited time to make a judgment, and it might therefore be argued that such a match is purely "analytic" in Brunswik's sense. At the same time, for the moving shape situation at the lowest velocity the subject has one and a half minutes during which the judgment may be made, yet the result suggests a totally different attitude. By attitude is meant a set of sensori-motor dispositions rather than a conscious mental state, since introspective comments volunteered by subjects in the course of the experiment tend to suggest that variations in the mode of appearance of the shape do not greatly influence their actual choice. Thus during the experiment it is common for subjects to state that at one moment they see a circle "turning round" and later to report that this appearance has been replaced by that of a circle being "squeezed in and out." However, examinations of scores made during these different experiences failed to reveal any significant differences. From the frequency histograms for four velocities it may be seen that there is a shift in the angles recorded in matches, from between −1° and zero at the lowest velocity to a distribution approaching the normal around 7°, at the highest. The rather wide distributions shown in the histogram are due not to variation in individual scores, as is shown by Table II, but arise from the nature of the constancy phenomenon.

Graph of Shape Matches with Continuous Movement of Stimulus



Each individual tends to have his own range of perceptual constancy; hence whilst means of pooled scores show large variations, grouping of scores, which ignores absolute magnitudes and records only changes in constancy with variation of some determinant, such as velocity of change, results in a smooth curve with small dispersions.

The histogram shows the whole range of possible scores, and it may be seen that at the lowest velocities the distribution of scores tends to be skewed sharply toward the zero (visual angle). It is obvious that there cannot be a normal distribution around the zero since this would correspond to the occurrence of "negative constancy."

It will be noted that the space error tends to fall with the increase in velocity. This error occurs, as might be expected, according to the direction in which the shape is rotating. Few subjects show anticipatory judgments, the general tendency being towards lateness in judgment. At a velocity higher than those covered in this experiment the space error becomes negligible. At this point, in the region of about 10 seconds per sweep, constancy reaches a level from which it shows no continued improvement. It would seem that around this velocity the perceptual situation reaches its greatest stability. It is also worth noting that it is in the region of higher velocities that subjects experience the reversals in the mode of appearance referred to above.

Whilst the range of velocities investigated were restricted to an upper limit of a 30-second sweep rate only by limitations in the available apparatus, it is nevertheless clear that there is an absolute upper limit to the velocity in this type of phenomenon above which the reactions of the subject will become hurried and confused. Such an experiment would then be investigating the speed and type of reaction by the subject rather than determinants of visual constancy. Consequently the range of experience to which these results apply must necessarily be limited.

In conclusion, the coefficient of variation has been calculated for Table II as it is misleading to compare the changes in dispersion by a simple measure since with increase in constancy the range of possible variation in match is also increasing. Twelve subjects, six of each sex, were used in the experiments, only two being students of psychology and five of the remainder university students. Two suffered from visual defects (corrected by spectacles), the rest having normal vision. A thirteenth subject was rejected as anomalous, but provided an interesting case by giving scores of negative constancy in Experiment I and zero scores in Experiment II. Subsequent study showed that this subject was unable to visualise the frontal-parallel ellipse as such in the dark space, but was compelled to experience it as a circle at some angle of tilt.

V

DISCUSSION

It would seem that the class of changing shapes, in cases where the rate of change is directly perceptible, constitute a distinct class of shape appearances in which many of the conventionally assumed "cues" or "gradient variables" do not have the importance they can be demonstrated to have in so-called "normal" perception. In the case of stationary shapes it would seem that removal of gradients such as texture, surface, illumination, etc., together with the surrounding field or setting negates the perception of orientation, which together with outline makes possible constancy of shape.

In such situations the perception of motion, a change of shape occurring in a regular manner, introduces fresh variables of the type listed above, namely, sloped gradients of retinal sweep and the skewing of binocular images. But there would appear to be strong objections to accepting such gradient variables as sole explanation of the phenomenon. Whilst it may be true that gradients of retinal sweep contribute to a judgment of tilt, such a variable cannot of itself explain why perception of motion is ascribed to orientation. Why should not the shape be seen as being "squeezed in and out" rather than "turning round"? In addition, such explanation leaves unaccounted for the fact that when this former experience does occur, constancy is not thereby affected. To make such an account acceptable it becomes necessary to invoke a learning process in order to "interpret" such a variable. But whilst putting forward a theory of gradient variables to account for constancy of perception, Gibson explicitly rejects a learning process as its concomitant, thus: "We do not have to learn that things are external, stable, rigid and spaced about the environment for these qualities may be traced to retinal images or to reciprocal visual-postural processes . . . the world does not seem to expand as we move forward in the environment although the retinal image does. The expansion is a stimulus correlate for moving forwards . . . the face of an object does not look compressed when seen at a slant since the compression is a correlate of its looking slanted" (Gibson, 1950, pp. 187, 190 ff.). This appears to answer the question by ignoring it, since there is no account of how these two sets of stimuli ever come to be correlated. Similar criticism of Gibson's views has come recently from Ittelson (1951), in the course of discussion of problems of size and distance perception.

The gradient of binocular disparity has even less explanatory value in the present case since the results in Table II were obtained using monocular vision; this variable can be at best only contributory.

Before passing to discuss an alternative line of explanation, it might be noted that in experiments of this kind a critical objection may be made, that what is responsible for the maintenance of constancy is the real, perceptual knowledge of the continuing identity of the circle possessed by the subject, and which he unconsciously "takes into account" when making a judgment. That this is not the case seems to be demonstrated by the results of Table I, where the subject also has perception of such a continuing identity, apparently without this knowledge affecting his immediate judgment.

The writer is indebted to Professor R. C. Oldfield for valuable suggestions as a result of which the following hypothesis is tentatively put forward.

In the present case not only have normally concomitant gradients of stimuli coming from the object itself been eliminated, but as far as possible those stimuli contributing to the perception of the surrounding field also. This would tend to prevent normal integration of object and field, or "figure and ground" into a perceptual whole, and under conditions of monocular vision there will be no true perception of depth, and hence no orientation or putative placement of the tilt of an object. In fact, as was suggested by the case of the thirteenth subject, there may even be illusory experiences equally legitimate to a true perception on the basis of the available data.

What seems clear is that the appearance of such a stimulus is purely phenomenal; there is no substantial "object," that is, a real subsisting entity behaving according to any general law. There is simply "an appearance." In the absence of supplementary cues a direct match between this phenomenal shape and another which is a projection of it will approximate to the retinal stimulation.

It is suggested that the intervention of motion in the manner of regular increase and decrease of the shape, taking place under limited ranges of speed, operates to "create" the object as a real and subsisting entity. In this way, analogously to the various perceptual phenomena demonstrated by Michotte, there is some interaction between the shape as a stimulus, and what is happening to it in a directly perceptible way, which gives rise to an object configuration, just as in the Michotte situations interation between perceptual "objects" give rise to "configurations of causality."

In a similar way, the "creation of an object" as described above may be noted in the case of "Lissajous figures" seen on the screen of a cathode ray tube. When two frequencies are applied to the pairs of plates in such a tube it is possible to generate a closed figure, from the simplest case of an ellipse to complicated closed rings having a number of crests ("Crown" figures). As compared with the simpler figures the more complex ones possess, whilst stationary, some degree of apparent depth; yet here the intervention of motion can bring about a great increase in the perfection of the illusion. For if by means of a continuous phase shift the crests are allowed to progress around the ring figure, the whole configuration takes on the aspect of a three-dimensional object to an astonishing degree of reality. (Fisichelli 1947): (Weber 1930). And it is suggested that a shape constancy experiment utilising the outline figures produced by a cathode ray tube could be made to yield similar results to those discussed in the present paper.

In conclusion, it will be apparent that the type of explanation offered here does not claim to be an account of the way shape constancy actually operates. It would seem that a genuine account could come only from a full genetic study of the constancies in the course of individual development. In the absence of this it is only possible to speculate on the means by which the factors discussed above are integrated in the process of perception.

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LEARNING OF A SERIAL TASK BY DIFFERENT AGE GROUPS

BY

H. KAY

*(The Nuffield Research Unit into Problems of Ageing,
The Psychological Laboratory, Cambridge)*

In a serial learning task given to subjects whose ages ranged from twenty to seventy, a decline in performance showed itself as a loss of speed in the thirties, a loss of accuracy in the forties, and a marked loss of both speed and accuracy in the fifties and sixties. Experiments on recall and reorganization of material verified that older subjects were forgetting more quickly than younger, but that one of their main difficulties was an inability to substitute new reponses in an already mentally formed sequence.

Older subjects were not introducing new learning features but accentuating normal tendencies, such as the repetition of the same error at the same serial position. In so far as subjects learned their repeated errors, final learning involved unlearning, with the often observable phenomenon of the previously learned error serving as the cue for the more recently learned correct response. In terms of schematic concepts of mental functions, serial learning difficulty was not so much the formation of a general "scheme" but its subsequent amendment. Preformed habits influenced both the learning procedure and its results, particularly in the constraints and expectancies which the making of one serial response had upon another.

I

INTRODUCTION

This experiment had two aims; it was intended not only to investigate how the learning of a simple series was influenced by age, but also to see if an examination of the influences of the age variable had clarified some of the problems in learning studies.

The reason why these aims should be so interlinked might be mentioned here, since the point has some bearing on the relations between the "psychologies"—if not the psychologists—of the applied and the theoretical fields. It was anticipated that studies of ageing might throw some light on the essential and non-essential features of learning experiments, either by exaggerating normal tendencies or by introducing new ones It was the first of the alternatives which was so outstanding in this experiment; older subjects were emphasizing, generally by repetition, features which in some degree were common to all age ranges, and this very exaggeration made it easier to appreciate what were the outstanding difficulties in learning the series.

To illustrate how the interlinking of the two aims worked in practice, two examples of this exaggerated performance will be briefly indicated. The series to be learned was very simple in order that all older subjects could master it within the experimental period. Yet some subjects took so long to learn this short series that it was not only obvious when their progress was held up, but it became possible to study a process which could most fittingly be called the unlearning of previously learned responses. It was because of older subjects' extended efforts to learn in this situation where the number of possible responses was so limited, that the nature of this unlearning could be scrutinized.

The second example is given because it is related to recent work which has brought out the inadequacy of examining serial performances as if they were a succession of discrete stimulus-response units. Some of the experimental findings in such work have stressed that no matter how strictly the conditions are governed, factors outside the experimenter's control are likely to influence the result. Whitfield (1950) illustrated these influences by proceeding to a logical end-point where the answers were given to imaginary questions. In the present paper an effort is made to indicate how frequently similar extraneous influences were actuating responses at some serial positions. Again it was a consideration forced upon the experimenter by the exaggerated performances of older subjects.

Thus the discussion in this paper, though conducted at a descriptive rather than an explanatory level, does aim to clarify some of the characteristics of serial learning, in addition to showing how their expression varies with age.

II

EXPERIMENTAL METHOD

Description of Apparatus.

The subject was presented with a row of ten lights positioned above ten morse keys, so that each light has a morse key just below and in front of it. Both the lights and keys are spaced two inches apart and are numbered one to ten.

A system of post office relays allows any series of lights, and any length of series within the ten lights to be set up. The key connections to the relays are also interchangeable so that any key can be made to control any one light. When a "correct" key is pressed it puts out the light which is showing and puts on the light which is serially connected to the last one. Every key is recorded separately.

Subjects.

Fifty subjects took part in the experiment, ten belonging to each decade from twenty to seventy. As far as possible in selecting subjects, those of each age group were matched for occupation and social background; seven of the ten represented business or the professions, and the other three were skilled or semi-skilled workers.

Experimental Instructions.

The apparatus was explained and demonstrated to a subject before telling him what he had to do. For the first part of the experiment he was asked to use only the keys 1–5. He had to find out and then learn a particular sequence of keys so that he could repeat it without a mistake. When this correct sequence was followed a light changed every time a key was pressed. Typewritten instructions were left with the subject; these were as follows:—

INSTRUCTIONS TO SUBJECTS

How the apparatus works.

When the "correct" key is pressed the light which is on goes out and another light comes on.

When an "error" key is pressed nothing happens.

In any one sequence one key will only put out one light.

What you are asked to do.

In this series you will only use the lights and keys 1–5.

The light will start at 1. You are asked to find the correct key which will put out this light and put it on at another point. You then find the next correct key for the light which has come on, and you repeat this procedure until the light comes back to its first position. This ends the first trial.

You rest for 15 seconds and then are asked to begin again, trying to remember and to hit only the correct keys.

You continue to repeat the sequence until you have learned the key series and can perform it without a mistake for two consecutive trials.

DESIGN OF EXPERIMENT

Stage I. Learning of Series I and II.

The subject learned a series of five keys, as instructed above. In series I the first five keys were used, in the order 2, 4, 3, 1, 5; in series II the other five keys in the order 7, 6, 10, 8, 9. Subjects were told that the order of keys in series II differed from that in series I. Both series were learned to a criterion of two consecutively correct trials.

Stage II. Relearning of Series.

After he had learned the second series the subject was asked to repeat the first series. If the recall was not correct at first, he continued until he had completed one correct trial. The second series was retested after the first.

Stage III. Alternation of Learned Series.

Finally the first lights in *both* series I and II were put on together. A subject was asked to begin with the first move from series I followed by the first move from series II, and continue the alternation between the series until he had completed both. Two trials per subject were made under these conditions.

For this stage subjects had to construct a new serial order out of the two previously learned sequences. A great deal of interference was anticipated in such a performance. It was hoped that the task would give some indication of the relative difficulty, and the nature of it, for different age groups.

Subjects never knew the full extent of the experiment until they had completed it, nor were they told that there would be any relearning or alternation tests until they were asked to do them.

The way in which certain terms will be used can conveniently be given here.

Trials.—A trial was measured from the time when the first light was switched on to when the subject pressed the last correct key in the series. (This returned the light to its original starting-point and after 15 seconds another trial began.) Thus in a single trial we had a measure of the time taken, and the errors in making the five correct responses.

Errors.—Any number of errors might have preceded the correct response at each position.

First errors.—Limits this number to the *first* error response only at each serial position, i.e. in a five-item series there could not be more than five such errors per trial.

Repeated errors.—A first error made at the identical serial position on more than one trial. This measure excludes the first time the error is made.

Serial position.—Serial position referring to the temporal order of a response within the series; and

Spatial position.—Spatial position referring to the actual place of the keys on the keyboard are frequently contrasted with one another.

The three scores, trials, learning-time and errors are not independent. The number of trials is to some extent dependent upon the number of errors when the subject is learning to a criterion of two correct trials. Learning-time depends partly upon the number of trials and partly upon the general tempo of performance.

III

RESULTS

A. Differences in Performance Between Age Groups.

Stage I.—The learning-times, the number of trials and the number of errors for both series are given in Table I and graphed in Figure 1.

The thirties made only slightly more errors and took slightly fewer trials than the twenties. Their overall learning-time was, however, greater than the twenties, because their tempo of performance as indicated by their time per trial was slower. This is further analysed in the section on learning times.

The forties took more trials and made significantly more errors than the thirties, but their learning-time was not proportionately greater—here accuracy had fallen off more than speed.

Above the forties all three scores, errors, time and trials increased rapidly. The sixties were taking more than twice the trials, making more than three times the number of errors and spending three to four times longer over learning the task than the twenties.

TABLE I

LEARNING-TIME, NUMBER OF TRIALS AND ERRORS FOR AGE GROUPS IN SERIES I AND II

Age group (10 subjects per group)	*Trials*		*Time*		*Errors*	
	Means (in secs.)	*S.D.*	*Means (in secs.)*	*S.D.*	*Means (in secs.)*	*S.D.*
			Series I			
20–29	6·0	2·32	71·8	21·10	16·7	7·34
30–39	4·9	2·00	87·0	22·29	18·4	14·48
40–49	5·9	2·45	85·4	22·23	23·1	9·79
50–59	10·4	4·59	193·1	50·89	39·5	25·10
60–69	12·3	5·91	238·3	93·48	53·9	37·30
			Series II			
20–29	3·9	1·64	42·4	13·03	11·8	11·98
30–39	3·9	2·73	60·6	21·26	12·9	7·30
40–49	6·2	3·40	81·0	22·67	24·8	16·10
50–59	8·4	4·69	155·1	60·89	29·8	22·41
60–69	9·6	6·16	207·1	74·99	46·6	41·25
			Totals for Both Series			
20–29	9·9	2·38	114·2	22·62	28·5	15·33
30–39	8·8	4·31	147·6	41·95	30·3	18·40
40–49	12·1	5·11	166·4	34·59	47·9	21·82
50–59	18·8	6·85	348·2	92·07	69·3	44·43
60–69	21·9	11·30	445·4	155·30	100·5	69·91

All figures are up to and including first correct criterion trial. The normal deviate in all the tables has been calculated in accordance with the formulae given by Kendall (1948) for the rank correlation coefficient.

Errors: Ranking all subjects by age ranges.
Normal deviate = 4·20, $P < 0{\cdot}001$.
Dichotomy: Subjects in thirties and forties.
Normal deviate = 2·08, $P < 0{\cdot}05$.

Time: Ranking all subjects by age ranges.
Normal deviate = 4·48, $P < 0{\cdot}001$.
Dichotomy: Subjects in forties and fifties.
Normal deviate = 2·46, $P < 0{\cdot}02$.

Time per trial: Dichotomy: Subjects in twenties and thirties.
Normal deviate = 2·23, $P < 0{\cdot}05$.

The variability of performance increased greatly with age, as indicated by the rise in the standard deviations for all three scores. In so far as these standard deviations are roughly proportional to the corresponding means, it can be said that the relative variability of the different decades is not greatly increased But in absolute measure the variation between subjects in the higher age ranges is very much greater than in the lower, and this produces some overlap in scores between individuals of different age groups. That this is not marked can be seen by the highly significant figures when the errors or the times of all subjects are ranked by age groups.

Stage II: Relearning.—The relearning data are given in Table II.

The decline in performance is similar to that in the original learning except that the thirties are now significantly less accurate as well as slower than the twenties.

FIGURE I

MEAN TIMES, ERRORS, AND TRIALS PER SUBJECT FOR LEARNING BOTH SERIES (excluding criterion trials)

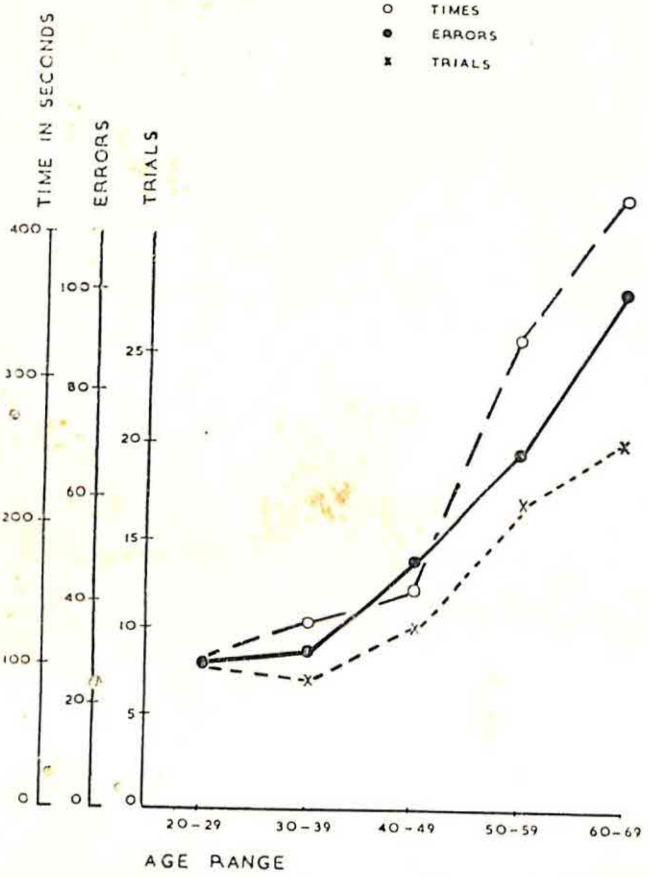

TABLE II

RELEARNING TEST FOR BOTH SERIES

Age groups (10 *subjects per group*)	*Total Relearning time* (*in secs.*)	*Trials*		*Total errors*	*How often series was correctly recalled on first trial.* (*Maximum possible* = 20)
		Total	*Total incorrect trials.*		
20–29	115	23	3	7	18
30–39	333	44	24	40	12
40–49	376	42	22	77	9
50–59	842	56	36	119	4
60–69	878	66	46	166	2

Total figures include first correct criterion trial.

Errors: Ranking all subjects by age ranges.
Normal deviate = 4·83, $P < 0{\cdot}001$.
Dichotomy: Subjects in twenties and thirties.
Normal deviate = 2·38, $P < 0{\cdot}02$.

Time: Ranking all subjects by age ranges.
Normal deviate = 4·56, $P < 0{\cdot}001$.

Results at this stage depend mainly on recall, as impaired by the length of time since learning a series, and also by the retro-active effects of subsequent learning. This decline of the thirties stresses that, however comparable their learning was to the twenties, their forgetting was certainly quicker.

The first errors which were made at the relearning stage were mostly repeats of the first errors which the subject had made during the initial learning. This was verified by checking the agreement between the modal error which each subject made for each serial position at the two stages of the experiment. Of the total number of modal errors in the relearning, 77·1 per cent. agreed with the subjects' modal error in the learning.

Stage III: Alternation of Series.—The data for the third stage of the task (alternation of items from the two previously learned series) are given in Table III. Aggregate time and errors are shown and as these are for the same two trials, the time per key struck is also given. Here, as in the initial learning the tempo of older subjects is markedly slower. The fifties and sixties found this a very difficult task—only two out of their forty trials were correct. In alternating the two learned series a subject had not only to select and differentiate between items in a manner apparently akin to that required in the original learning; there was a further difficulty, which the older subjects in particular experienced, and which could be mainly ascribed to two causes: firstly, the stress which this selection imposed upon memory. The subject had to retain in two separate series the responses he had just made, and at the same time recall the serial responses which were to follow; and secondly, negative transfer arising because a response no longer followed in its previously learned serial order, but was replaced by a new response.

TABLE III

ALTERNATION OF SERIES
(two trials only)

Age group (10 *subjects per group*)	*Total time* (*in secs.*)	*Errors*	*Number of correct trials* (20 *possible for each age group*)	*Times per key struck*
20–29	489	28	9	2·14
30–39	484	55	7	1·90
40–49	594	75	5	2·16
50–59	763	77	2	2·72
60–69	1,179	152	0	3·35

Errors: Ranking all subjects by age ranges.
Normal deviate = 4·07, $P < 0{\cdot}001$.

Time: Ranking all subjects by age ranges.
Normal deviate = 5·01, $P < 0{\cdot}001$.

B. *Increase in the Learning-time with Age.*

Subjects had been given no instructions whether learning-time or errors were being scored. From the nature of the task it is likely that they would have been led to pay more regard to accuracy than to speed, since there was no indication at what speed they should perform, but there was immediate indication whether their response was right or wrong. So far as could be judged both on these grounds, and from observations by the experimenter, subjects were setting their own tempo of performance, i.e. the time taken to strike each key. The variations with age in preferred tempo can be illustrated by considering "time per key struck" on the first trials only, during the initial learning of series I and II.

TABLE IV

TIMES FOR FIRST TRIALS ONLY

Age group (10 subjects) per group)	Series I Time per first trial (in secs.)	Series I Time per key struck (in secs.)	Series I First trial time as per cent. of total learning time	Series II Time per first trial (in secs.)	Series II Time per key struck (in secs.)	Series II First trial time as per cent. of total learning time
20–29	170	1·43	22·3	177	1·53	39·2
30–39	290	2·36	31·5	212	1·78	32·7
40–49	260	2·04	29·0	232	1·86	27·6
50–59	338	2·40	16·9	269	1·95	16·7
60–69	339	2·40	14·0	311	2·22	14·7

Decrease in learning-time between series I and II.

Subjects in the twenties and thirties: Difference between the two means for the decrease of time between series I and II: $t = 3{\cdot}022$, $P < 0{\cdot}01$.

It will also be seen from Table IV that there was a striking contrast between the performance time for the twenties and the thirties. For the first trials in series I the twenties took 170 seconds and the thirties 290 seconds; for the first trials of series II the twenties were the only age group to increase their time while the thirties decreased theirs from 290 to 212 seconds. Taking as the variable the decrease in time from series I to series II the mean value for the twenties was found to differ significantly from that of the thirties.

These facts suggest that from their experience of performing the first task the younger subjects recognized that their initial speed was not advantageous to their learning. Support is given by the direct observation of the procedure which these subjects adopted, and by their comments whilst carrying out the task. At the first trial they were willing to begin without any clear idea of how they were going to proceed, and allowed the actual performance to dictate their technique. As a result of the practice effect from series I they completely redistributed their learning-time in series II, spending 39 per cent. of their total time on the first trial.

The thirty to fifty age groups were much more painstaking in their approach, carefully examining display and controls both before and during the first trial. They asked many leading questions such as, "Then I don't need to look at . . ." etc. They were seeking answers not only about the apparatus but how they were to perform on it, so that they tended to start with some method for doing the task. The effect of practice for the thirties, and for all the age groups except the twenties, was that they began the second series at a quicker rate than the first, but it is of interest that in all these age groups the ratio of the first trial time to the total learning-time is constant over the two series. (See Table IV, percentage column figures). Only the twenties increase this time distribution on the second series.

From the figures in Table IV indicating that for the fifties and sixties the "time per key struck" is about one and a half times that of the twenties, it appears that the slower tempo of performance of the older subjects was contributing a little to their longer overall performance time. But this slower tempo would not account for the whole of their increased learning-time which is three to four times that of the twenties. A study of the type, the number and the repetition of errors gives some explanation why the time increase was so great.

It is being stressed that this increase may have arisen from more than one contributory cause, since there has been a tendency in ageing studies to measure overall time and contrast it with the quality of performance. Sward (1945), for example, says, "Other things equal, age has the effect of impairing the rate far more than the quality or the accuracy of the mental operations here considered."

It may well be that scores of accuracy on such tests between different age groups are often more comparable than their overall time scores, leading to conclusions such as the above. But the different time scores between age ranges may be indicative of just those changes in "the quality of the mental operations" which their identical scores for accuracy are concealing; that is to say different age groups may have gained the same end score of accuracy by very different "mental operations." There is a hint of this in the same error results but different time scores for the twenties and thirties.

C. *Characteristic Errors in Learning this Serial Task.*

So far we have considered the methods by which this task was executed only in respect of quantitative criteria—number of trials, number of errors and time taken. In this section we shall be more concerned with how performance was qualitatively modified by certain characteristic errors of serial learning. We shall be considering two types of learning mistakes—those arising from various expectancies which subjects had about the *spatial* randomization of responses, and those occurring because subjects anticipated items in the partly learned serial order.

(1) *A Spatial Expectancy Error.*—The errors in series I and II do not conform to the usual distribution within a series where the maximum point is at the centre. It will be seen from Figure II that at some points in the two series there is a striking reversal in the distribution of errors; for example, the second position has the minimum number of errors in the first series and the maximum in the second. In order to examine how far this tendency had persisted throughout the course of learning the *first* trial of all 50 subjects was analysed; the number of errors made at each serial position is given in Table V. The distribution of errors in the first trial, particularly that on series II, is similar to that for the total learning of the series; the second position still has a strikingly different number of errors for the two series.

We cannot account for this result by any specific interaction between series I and II; the maximum number of errors still occurred on the second serial position when series II was given independently. The explanation seems to be that a subject brings to the learning of a serial task certain expectancies and these are of a general kind, independent of a specific transfer from a previously executed series. The most obvious illustration of this is the subject's idea of alternation. When he has hit a key at one end of the board he expects the next response to be towards the other end. He does not anticipate that the next key may be adjacent to the one he has just struck. As one subject put it: "I was working on the principle that if I hit a correct key at one end, then I tried the other."

It is of some note that, coupled with this tendency, subjects seemed to find it easier when the movement involved going from left-to-right than from right-to-left.

If we check the number of errors made in learning keys which involved these contrasted movements, we find the errors scored were 946 from left-to-right and 1,506 from right-to-left. However, as these movements could not be equated in their serial positions, Table VI only gives the figures for the second and third serial positions.

FIGURE II

Total Error Responses at each Serial Position in Learning Series I and II (50 subjects)

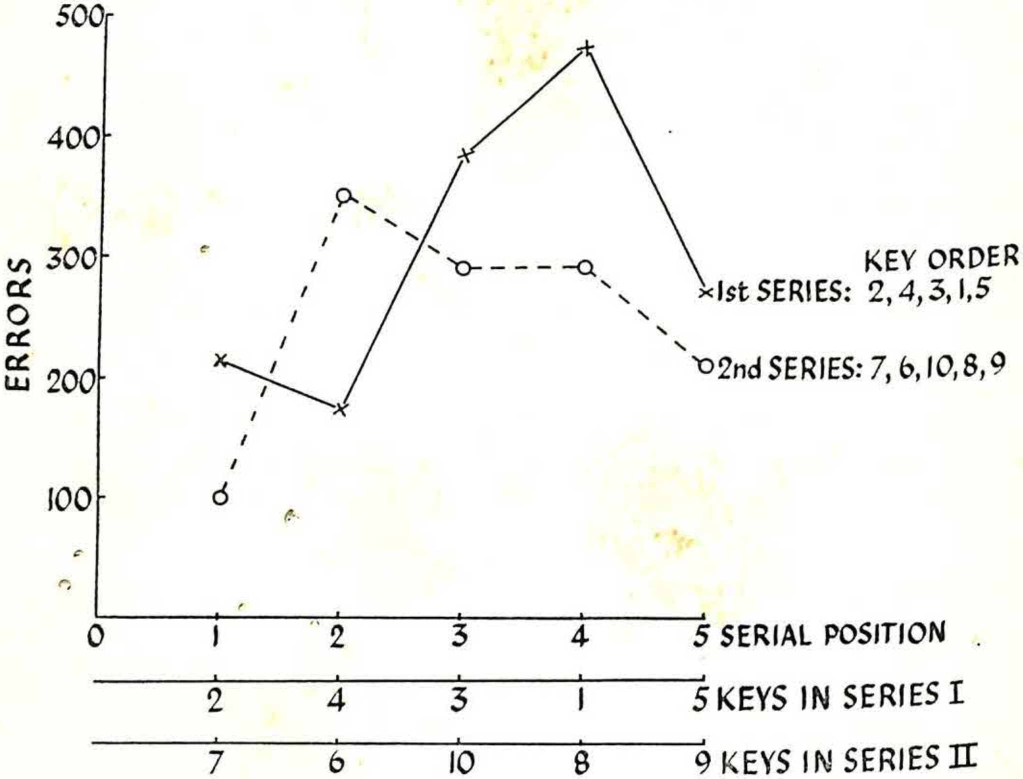

TABLE V

Error Responses at each Serial Position on First Trial

	Series I					*Series II*				
Serial position	1	2	3	4	5	1	2	3	4	5
Correct key	2	4	3	1	5	7	6	10	8	9
Number of errors ..	60	73	92	79	94	56	128	93	68	51

Examples such as these two—the subject's idea of randomness and the left-to-right contrasted with the right-to-left movement—suggest two points. Such features emphasize that in considering the effect of serial position upon learning, it is necessary to allow for the influence of more than the ordinal position of the item within the series. The idea of serial position needs to include the probably more fundamental point of the constraints and expectancies which the making of one response will have upon the next—one action is giving the "direction" or cues for the next. It is in its relational context both to previous and to anticipated responses that the ease or difficulty of learning an individual response must be understood.

At the same time it is being stressed that these specific predilections for certain serial items or for directions of movements merely exemplify more generalised tendencies, and they have been put forward to illustrate the more general point. It seems that when subjects have to make a choice at any particular serial position they do not give equal weighting to all possible responses. They bring to the task, as illustrated by data from their first trial, definite preconceptions as to what it would demand of them, and how they would comply with these requirements. And their responses throughout learning show that, far from having only an initial influence, such preconceptions have a lasting effect. These then are variables which are inherent in adult learning and in so far as it is probable that the older subjects have the more definite preconceptions their learning is the more likely to come under such influences.

TABLE VI

ERRORS MADE IN LEARNING LEFT-RIGHT AND RIGHT-LEFT MOVEMENTS

	Left to right errors	*Right to left errors*
Second serial position	176 (key 4)	350 (key 6)
Third serial position ..	290 (key 10)	385 (key 3)
Totals	466	735

(2) *Anticipatory Errors.* These errors—"one of the very widespread phenomena of learning" (McGeogh, 1942)—have been discussed by learning theorists of all schools. (Spragg, 1936, briefly reviews the literature on the subject.) Normally the name includes all responses which are subsequently correct in the series. Here, because the series is short, we are only considering the immediate anticipatory error, i.e. the first error response made at any serial position. In calling these errors anticipatory the intention is to emphasize their relation to responses which are subsequently correct in the series; it is implied that the subject is aware of some serial relationship between the responses which have been made and those which are still to be made.

We shall consider in this section the part which anticipatory errors are playing as learning proceeds. Of all errors they were the most common and the most persistent. This is illustrated by the learning record for a single subject, shown in Table VII. It will be seen that in series I, where the correct order was 1, 5, key 5 was repeatedly hit at the serial position of key 1; in series II where the order was 8, 9, key 9 was hit at the serial position for key 8. In order to bring out the generality and persistency of this type of mistake, the total distribution of first errors is shown in Table VIII. The error keys are placed in serial order so that the immediate anticipatory error is shown first at each serial position. In nearly every instance most mistakes were made at this first point; the one exception was the first serial key in series I. The distribution over all the keys was anything but random; there was a definite tendency for those responses which were serially nearer to the correct response to occur more frequently.

In an attempt to find out how this process of concentrating upon the anticipatory errors developed during the course of learning, the total distribution of Table VIII

TABLE VII. INDIVIDUAL SUBJECT'S LEARNING RECORD FOR SERIES I AND II

	Series I					*Series II*				
Serial Order of Keys	2	4	3	1	5	7	6	10	8	9
TRIALS 1	—	3	5	—	2 3	—	8 7 9 7	9 7 8	6 7 9	7 6 10
2	1 3	—	—	5 4 3 2	3 2 4	9 8	9 8 10	9 8 7	6 7 9	7 6 8
3	1 3 4	5 3	2	5 4 3 2	4	—	9 8 10	9 8 7	9 7 6	7 6 10
4	1 4	3 5	2	5 4 2	3 4	—	9 8 10	9	—	7 6 10
5	1 4	—	5	5 4 2 3	—	—	8 9 10 9 8	8	—	6
6	1 3 4	—	—	5 4 2 3	—	—	9 8	—	9	6
7	1	—	—	5 4 3 2	—	—	9 8 10 9	—	—	—
8	—	—	—	5 2 4	4	—	—	—	9	7 6 10
9	—	—	—	5	4	—	—	9	9	7
10	—	—	5	4 2 5	—	—	9	—	9 7	—
11	—	—	—	5	—	—	10 9 8 9	—	7 9	7 6 10
12	—	—	—	5	—	—	9 8 9 10	—	9	—
13	—	—	—	—	—	—	10 9 8	—	9	—
14	—	—	—	5 4 3 2	—	—	10 9 10	9	—	10
15	—	—	—	5	—	—	10 9 8 10 8 7	—	9	—
16	—	—	—	5	—	—	10 9 10 8	—	—	—
17	—	—	—	5	—	—	9 10 8	9 8	9	—
18	—	—	—	4 5	—	—	9 10	—	9	—
19	—	—	—	—	—	—	10 9	—	9	—
20	—	—	—	—	—	—	—	—	9	—
21	—	—	—	—	—	—	—	—	9	—
22	—	—	—	—	—	—	—	—	—	—
23	—	—	—	—	—	—	—	—	—	—
Total errors at each serial position	13	5	5	41	11	2	56	15	24	22
Total 1st errors at each serial position	6	3	5	16	7	1	17	8	16	10

was further analysed. Table IX, laid out in the same manner as Table VIII gives the percentage number of errors at each serial position for three different stages during the course of learning—the beginning, the middle and the end of each individual's learning record for each particular serial position. If we compare these figures with the percentage figures for Table VIII we can see that initially (row A) there was little or no tendency to agree with the anticipatory order: we have an almost random distribution. By the middle stage of learning (row B) the distribution of errors was conforming to the serial order, and this tendency to concentrate errors more and more upon the key which would be correct at the next serial position was at a maximum in the last stages of learning (row C). The process was not one which developed and then declined, rather it was something which developed and was most rigid in the later stages of learning. A subject's error responses were the most predictable at the point just before learning was completed. These results imply that the length of time which had elapsed, and the number of events which had taken place, since a key was correctly struck, partly determined how often that key was tried at any serial position. From observing subjects' performances when these anticipatory errors occurred it was often possible to distinguish one of two procedures being followed. (It is not implied that these were always distinct, and indeed it is thought that the second succeeded the first as learning proceeded.)—

(1) The first procedure, which characterised the early stages of learning, was generally slow and hesitant as if a subject, not knowing the right key, pressed one which he had not used recently.

(2) The second was a more definite selection and typical of the later stages of learning, when the subject had established some serial order and was anticipating responses which he knew he had yet to make. A key was selected because it was thought to be the next serial response and in fact it proved to be the next but one. The speed with which this wrong response was made, and a subject's surprised reaction when he discovered that it was wrong were indicative of this stage of learning. This point is considered more generally in the discussion.

C. *Repetition of Errors.*

The discussion of anticipatory and spatial expectancy errors, giving examples of the disproportionate frequency with which certain keys have been hit at some serial positions, provides some preliminary indication how far subjects will continue to repeat the same mistakes. It should be stressed that errors and error repetition were not differently distributed between age groups, but that older subjects were exaggerating repetitive tendencies which were typical of all age groups. In the fifties and sixties 76·3 per cent. of their *first* responses for each serial position were repeats of former errors. Some repetition is, of course, inevitable in a five-item serial task. Even so, an examination of individual learning records such as the one given in Table VII, or of the distribution of errors as shown in Table VIII, makes it fairly clear that this repetition is not due to any chance selection. There is an obvious tendency to repeat some errors at the same serial position in successive trials.

When an attempt is made to demonstrate this preference statistically the difficulty arises of assessing the chance probability of a particular key being struck at a particular point. This probability almost certainly changes during the course of learning when some of the responses are learned more quickly than others. A binomial method was adopted of showing for one serial position a marked concentration upon one error key as opposed to all the other error keys, which up to that point

TABLE VIII

TOTAL DISTRIBUTION OF FIRST ERRORS AT EACH SERIAL POSITION
(for all subjects)

Series I

Correct key (in serial order) . .	II				IV				III				I				V			
Error keys (in serial order) . .	4	3	1	5	3	1	5	2	1	5	2	4	5	2	4	3	2	4	3	1
No. of errors . .	11	31	63	20	78	24	16	3	84	62	34	8	145	49	43	—	71	23	22	4
Percentage distribution of errors for each serial position.	8·8	24·8	50·4	16·0	64·5	19·7	13·3	2·5	44·7	33·0	18·0	4·3	61·2	20·6	18·2	—	59·3	19·1	18·3	3·3

Series II

Correct key (in serial order) . .	VII				VI				X				VIII				IX			
Error keys (in serial order) . .	6	10	8	9	10	8	9	7	8	9	7	6	9	7	6	10	7	6	10	8
No. of errors . .	24	7	21	8	62	29	52	1	71	52	27	4	70	49	38	—	39	35	32	5
Percentage distribution of errors for each serial position . .	40	11·7	35	13·3	43·0	20·1	36·1	0·8	46·1	33·8	17·5	2·6	44·6	31·2	24·2	—	35·2	31·5	28·8	4·5

The correct and error keys are in their serial order so that the distribution of errors according to this order becomes apparent. The first error key (i.e. the immediate anticipatory error which is underlined) is the key which will be correct at the next serial position; the last error key is the one which has just been correct at the last serial position.

TABLE IX

Distribution of First Errors at Three Stages during Learning

(At each serial position the figures give the *percentage* number of errors on each key)

Series I

Correct key (in serial order) ..	II				IV				III				I				V			
Error keys (in serial order) ..	4	3	1	5	3	1	5	2	1	5	2	4	5	2	4	3	2	4	3	1
(A) Errors on first trial	11·1	13·9	58·3	16·7	66·5	26·1	4·8	2·4	24·4	53·3	15·6	6·7	25·0	33·3	41·0	—	54·0	10·9	35·1	—
(B)* Median errors	7·7	23·1	46·1	23·1	58·9	23·5	17·6	—	56·0	28·0	16·0	—	60·6	21·2	18·2	—	55·6	22·2	22·2	—
(C)† Last errors ..	17·6	29·4	47·1	5·9	70·8	16·7	8·3	4·2	58·9	26·4	11·7	3·0	82·5	12·5	5·0	—	63·1	36·9	—	—

Series II

Correct key (in serial order) ..	VII				VI				X				VIII				IX			
Error Keys (in serial order) ..	6	10	8	9	10	8	9	7	8	9	7	6	9	7	6	10	7	6	10	8
(A) Errors on first trial	34·4	21·8	34·4	9·4	14·6	29·2	53·7	2·5	43·2	2·5	25·0	6·8	35·3	20·6	44·1	—	16·7	58·3	25·0	—
(B)* Median errors	43·0	14·0	43·0	—	43·7	25·0	31·3	—	45·0	25·0	30·0	—	33·3	52·4	14·3	—	40·0	40·0	20·0	—
(C)† Last errors ..	43·0	14·2	42·8	—	50·0	29·0	21·0		57·1	35·7	7·2	—	50·0	35·3	14·7	—	45·5	27·3	22·7	4·5

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* Row B was plotted by considering each serial position separately for each subject. The median error was found for each serial position and the error key at that point was scored. Figures were summated for all subjects and the percentage distribution of errors at any one serial position was obtained.

† Row C followed a similar procedure to plot where each subject made his last error at each serial position.

in the series had not been correctly struck. The second serial position in series I was taken as an example: the correct key is No. 4 and the three error keys are 3, 1 and 5. The calculation was made on the assumption that all three error keys had an equal chance of being struck. When the probability for each individual subject in the fifty and sixty age groups was summated, it was found that key 3 as compared with the other error keys had been chosen significantly more often than chance. Using the method given by Fisher [1925, p. 99] of combining the probabilities, values here were $\chi^2 = 61{\cdot}55$, degrees of freedom $= 40$, $P < 0{\cdot}02$.

This demonstrated that there was discrimination upon one particular error response as opposed to another. An attempt was then made to show that a similar preference was operating when the choice lay between two responses which had not yet been correctly made in the series, but when one was an error and the other the correct response. The fourth serial position leaves the subject with two such responses: in series I this would be the anticipatory error, key 5, and the correct response, key 1. It will be remembered that the learning criterion was fulfilled when two consecutively correct trials were completed. But the difficulty arises that this serial position was generally the last to be learned, so that the striking of the right key at this position for two successive trials would have fulfilled the learning criterion, even if such responses were accidental. Thus, the number of incorrect attempts observed was almost certain to be higher than the number of correct attempts. The problem was solved in an indirect way. It was assumed that there was equal probability of a subject hitting either the correct or the error key (i.e. $P = \frac{1}{2}$ at each trial) and the chance of continuing for any given number of trials without hitting the right key twice in succession was calculated. It will be appreciated this was a strict criterion—it assumed no learning of the correct response by the subject. Even so, when the individual probabilities for the subjects in the fifty and sixty age groups were summated, it was found that the probability of obtaining by chance as many trials at this serial position as those actually observed was very small. The expected number of trials by chance was 6. The mean observed number of trials at this point was 8·9. Thus it can be said that during learning these subjects had shown a significant preference for the error rather than the correct key. ($\chi^2 = 60{\cdot}55$, degrees of freedom $= 40$, $P < 0{\cdot}02$.)*

This gives some statistical verification that a subject is choosing to make a particular error at a particular serial position in preference to both other error and correct responses. Of course, this is no new phenomenon, but can be substantiated by the results of other serial learning experiments. Lumley (1932), for instance, gives examples of increasing rigidity of responses as learning proceeds, as do McGeogh (1942) and Spragg (1936). It is only in the unusually persistent repetition of the older subjects that anything novel was introduced.

IV

Discussion

In so far then as these results are exaggerations of typical learning features observed by many experimenters, they can serve as the basis for the following observations.

Influence of Preformed Habits.

The experimental findings indicate that for all subjects there is a distinction in learning between the difficulty of retaining the correct items, and the difficulty of retaining items some of which may not be correct. The question why a subject should find it easier to learn certain error responses rather than their correct equivalents is as difficult as it should be revealing. Older subjects were certainly capable of establishing, relatively quickly, a series of consistent responses; in a comparatively short period they were retaining some facts. Differences in final learning-speeds between age groups were not to be explained completely by differences in rate of forgetting.

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* For this method and for much assistance in discussing the problem, the writer is indebted to Miss Violet Cane.

The results presented in the section on spatial errors suggest that one explanation why some items were learned more quickly than others was to be found in the preformed habits of subjects. This can be related to Bartlett's work on dominant detail where dominance seems so often to be established as a result of experience (Barlett, 1932). Subjects learned those items to which their preconceptions of the material and their general expectancies had directed their attention. Ageing studies, in their emphasis upon how far subjects attempt to relate instructions to their own experiences, do stress that a learning experiment and its material cannot be treated as isolated phenomena with independent and measurable objective difficulties.

These findings can be placed in the general context of experimental evidence illustrating how far subjects have their own preconceptions by which they make for themselves a structured field however unstructured it may seem to the experimenter. Of such work Skinner's (1942) analysis of the repeated guessing of alternatives, and Whitfield's (1950) "The Imaginary Questionnaire" are some of the clearest examples of the initial tendencies which subjects bring to a situation. The data of the present experiment show how these initial tendencies continue to influence subsequent learning, both in the manner of performance, no less than in the result.

Interaction of Items in Serial Learning.

Rote-learning of a series seemed to be a positional fitting together in a unified relationship of a number of originally discrete items. In studying how a subject assembled this material we have to allow for the interplay between items as they were combined within a total pattern. Every item became related to every other item, and was influencing its choice at any particular point. We have noted this influence at the beginning of learning where one response indicated, even dictated, the choice of the next, so that the ease of learning the next item depended upon how far its position conformed with such indications. Later, as learning proceeded there was a tendency for the subject, when he reached an unkown point in the series to put forward the learned items which were closely related to the unknown point. Generally, as we have seen, the subject did not repeat the correct responses he had just made, but selected responses which followed the unlearned item in the series —that is, he made anticipatory errors. The result was that the subject established a mental series which was shorter than the actual series, because two known items were related together to the exclusion of an unknown. If learning was to be completed this relationship had to be amended by adding to or substituting within the originally learned sequence. But the older subjects, having achieved some serial organization of material, tended to adhere to it.

It was this correction, demanding substitution within the sequence which was one of the weakest parts of older subjects' learning. Once incorrect responses had been learned, the subject had to learn new responses to replace them, whilst repeating the old responses which were correct. The learning of a new correct response at a previously learned serial point—here equivalent to the old stimulus—puts the learner in a negative transfer situation. The point emphasizes how far rote-learning does involve a correction of errors, and the essential difficulty of that correction when it necessitates a modification to the mental form which a learner had given to a series.

The Need for Amending Schematic Material during Learning.

If this is thought of in terms of some schematic concept of mental functions, the crucial learning difficulty was not so much the formation of a general "scheme," but the amendment of it. What the subject had schematically determined to be the response was predicted as the response, and with some subjects continued to be so predicted long after objective evidence had repeatedly shown it to be otherwise. It was in this sense that a subject could be thought of as trying to impose a schematic form upon the material, and being reluctant to amend it. It was a reluctancy that was obviously increasing with age.

From the point of view of a subject, it can be said that in rote-learning he introduced and, in so far as he repeated them, learned his own errors. Failure to modify the learned series led to continual repetition of the same responses. It was the paradox of this repetition that, whilst it strengthened the correct responses, it often seemed to be reinforcing the errors, which as we have seen, became more stereotyped and rigid. This was almost a vicious circle procedure, in that, if a subject was left to his own resources, he could only learn by further repetition, but that repetition was also strengthening the errors he was trying to eliminate.

The continual repetition of errors and the difficulty of their correction are, of course, not peculiar to rote-learning. Stereotyped procedures, which would parallel the continual repetition of errors have been frequently found in problem-solving and might be compared to the rigidity of behaviour reported by the psychiatrist The previous section tried to bring out how this difficulty of correcting mistakes in a learned sequence was a feature of rote-learning, since such correction and modification suggests an activity closer to problem-solving than to rote-learning as it has been traditionally conceived. It is felt that the results of this experiment stress the essential continuity between the two activities which have often been regarded as distinct.

There was evidence that subjects found their continual repeating of the same mistake a frustrating process; they were trying to establish one order of responses and the material would not allow it. Often they became so convinced that the apparatus was at fault when their predicted response was not correct that they accused the experimenter of changing the key sequence. Such indications of emotional stress would suggest that a full explanation of error repetition would have to consider how a subject becomes "involved" in his own solution. This experiment can provide only descriptions of behaviour, but at that level it would definitely support the argument that a subject after learning a wrong sequence tries to "make" it the correct one.

Learning Involves Unlearning.

Learning seemed to be achieved finally when the subject had so firmly established the correct serial responses that the error could be located in its relationship to them; the schematic material had become "articulated" (Oldfield and Zangwill, 1938). Without such repetition the subject seemed unable to select from within the interrelated items and identify the locus of error. It was noticeable that during the course of learning a subject gradually identified an error which he had made persistently, and only after that did he learn the right response to replace it. In performing the task at this stage the subject literally moved to press the right key via the wrong; his actions showed that the previously learned error was now serving as the cue for the more recently learned correct response. Thus one way of learning—and it was common enough although circuitous—was to link right and wrong together, so that it was *not* the isolated correct response which was learned, but the correct response related to an incorrect. The persistency with learned material of this correct to incorrect relationship, of the older to the more recent material, is again a fairly widespread phenomenon, and in a specialised field has been reported in prefrontal leucotomy cases.

For many subjects, therefore, learning was a process involving much unlearning and much amendment. As ever, the poorer performer was executing the harder task. He was spending much of his time trying to unlearn an error which he had himself brought into the task, and he still had to learn the correct response. A difference in the actual mental tasks which have been undertaken and achieved, seems to be some of the explanation for the pronounced individual differences which learning experiments always reveal.

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AN EXPERIMENT IN PROBLEM SOLVING

BY

J. W. WHITFIELD

(*Psychological Laboratory, University College, London*)

Trial-and-error problems are described in terms of "stimulus" difficulty, which is a measure of the number of possible modes of response left to the individual when all the information given is taken into account; and "phenomenal" difficulty, which is a measure derived from the individual's performance. An experiment is described in which three types of problem were presented to human subjects. In all three problems the stimulus difficulty was calculable, stage by stage, in the solution. The problems differed in this stimulus difficulty and also in the qualitative nature of the information provided—from unequivocal to conditional. It is shown that the qualitative difference of the nature of the information bears most relationship to phenomenal difficulty. Some observations are made on the modes of solution adopted, and further experimental work is suggested.

I

INTRODUCTION

The need for description of a task.

THIS experiment was performed as an attempt to describe in both quantitative and qualitative terms the behaviour of individuals faced with a "trial-and-error" problem solving task. It has long seemed to the writer that there is an inadequacy of description of tasks, and of stages in solution, in both problem-solving experiments and in learning experiments. Tasks are frequently described in terms of psychological theory or inferences from behaviour. As examples, the descriptive adjectives may be "trial-and-error" or else "insight." If, instead, we consider tasks as modified by knowledge of results in stages during the solution, we may find a more satisfactory mode of description, referring to the problems themselves, and not to theory nor even necessarily to behaviour. It may thereby become possible to establish an independent base line from which behaviour may be measured or described. Considering briefly the two types of problem mentioned above—"trial-and-error" and "insight"; it would appear that the "trial-and-error" problem is one in which there is a reasonably high probability of achieving some degree of success fairly early in the attempt at solution, and that both the correct and the incorrect elements of the partial success, if known and acted upon, constrain behaviour closer and closer to the completely correct solution. An attempt at solving an "insight" problem, on the other hand, is almost certain to be either completely right or completely wrong, and in the latter case, except for the narrow information that this specific attempt is wrong, very little information is gained. The problem remains approximately of the same difficulty until complete solution is obtained.

The term "difficulty" having been introduced, it would be as well to consider its meaning in this context. Its usual reference in psychology is derived from behaviour, measured by the mean time or mean number of trials required for solution or for fulfilling an accepted criterion—or alternatively, in terms of what proportion of a standard population under standard conditions obtain a solution. But it would seem possible, theoretically at any rate, to consider a more objective form of difficulty, associated purely with the problem and not dependent on the behaviour. To take an example, if we present a multiple-choice intelligence test question the difficulty of the question is dependent partially on the number of possible "answers" presented. This is merely stating that the task lies mainly in the choice; the judgment of the fittingness of the question to each of the possible given answers. Other things being equal, the "difficulty" is a function of number of answers given. This is, of course, only partially true, in the sense that behavioural "difficulty" can be modified without changing the number of answers—i.e. if all except one of the given answers are obviously absurd then the task is simplified. But this is an entirely different notion of "difficulty." These two types of difficulty may perhaps be named "stimulus difficulty," referring to the possible descriptions or measurements inherent in the form of the task, and "phenomenal difficulty" referring to the perceptual and cognitive aspects of the content or the mental processes required for solution. The analogy with terms in the psychology of perception is deliberate. As in the study of perception, "stimulus" is intended to be described in objective or physical terms, and "phenomenal" in terms of the behaviour of the observer.

As mentioned earlier, the case of problem solving adds the further complication that difficulty is going to be affected by partial success and knowledge of results. At first sight it would seem not easy to allocate this change of difficulty to either stimulus or phenomenal difficulty, in that knowledge of results requires appreciation by the observer to be effective. However, if it is considered as information now made available, to be added to the basic information given in the problem, it can clearly be treated as a change of stimulus difficulty.

Thus for the study of problem solving it is desirable that the problems chosen should be such that a measure of stimulus difficulty can be made, and that changes in stimulus difficulty in the process of solution can also be measured. The phenomenal difficulty should be measurable from the behaviour of individuals. Further, the problems chosen ought to be such that qualitative observations, which may hint at the modes of solution attempted or the mental processes involved should be capable of record.

Design of suitable experimental problems.

Simple derivatives of matching tasks were devised as problems conforming to the suggested requirements. Let us consider the simple task in which four objects are to be matched against four spaces—or four other objects. For a first trial, there are 4! or 24 ways of arranging the objects. Thus the initial stimulus difficulty can be considered as a function of 24. Of these 24 arrangements, one is completely correct. In this case the stimulus difficulty ceases to exist, the problem is solved. Six of the possible first-trial arrangements give two correct matchings and two incorrect. We have now a great deal of information. For example, we may know that A goes on α, B goes on β, C does not go on δ, and D does not go on γ. Altogether this information adds up to the statement that the solution must lie in interchanging the two incorrect objects. Thus, instead of the initial 24 possible arrangements, there is now only one possible arrangement. The stimulus difficulty has been reduced to some function of one. The phenomenal difficulty need not of necessity be similarly reduced—the objects may so closely resemble each other that it is almost impossible to remember which object was where. Eight of the possible first-trial arrangements have one correct matching and three incorrect. One of these is

$$\begin{array}{cccccc}
 & \textit{1st trial} & & \textit{2nd trial choice of} & & \\
\begin{array}{c}\alpha\\ \beta\\ \gamma\\ \delta\end{array} & \begin{array}{c}\underline{A}\\ C\\ D\\ B\end{array} & \begin{array}{c}\text{leading to}\\ \rightarrow\end{array} & \begin{array}{c}\underline{A}\\ \left\{\begin{array}{c}B \text{ or } D\\ B \text{ or } C\\ C \text{ or } D\end{array}\right\}\end{array} & \rightarrow \quad \text{either} & \begin{array}{ccc}A & & A\\ B & \text{or} & D\\ C & & B\\ D & & C\end{array}
\end{array}$$

—two possible arrangements, one correct, the other giving sufficient additional information for the correct arrangement to be the only possibility for the third trial.

And still further, nine of the possible first-trial arrangements are without any correct matches. But they still give information.

	1st trial		*2nd trial choice of*
α	B		A, C or D
β	C	leading to	A, B or D
γ	D	→	A, B or C
δ	A		B, C or D

which has nine possibilities. Tabulating the possibilities (continuing the correct solutions throughout) we have:—

(For the first trial the number of possible arrangements is 24.)

Trial 1		*Trial* 2		*Trial* 3		
Frequency	*No. of possible arrangements for Trial* 2	*Frequency*	*No. of possible arrangements for Trial* 3	*Frequency*	*No. of possible arrangements for Trial* 4	
1	0	12	0	23	0	All correct at Trial 4
6	1	10	1	1	1	
8	2	2	2			
9	9					

Thus we can see how the stimulus difficulty is changing, and the variation in change with the degree of success achieved.

The number of possible arrangements is not a very convenient measure to use directly. This will be obvious when one considers matching a greater number of objects, the distribution tends to be skewed very markedly, so much so that any simple average would not be meaningful and a transformed value is desirable. For the first trial, the number of possible arrangements is $n!$, where n is the number of objects to be ordered. This number is reduced trial by trial. It seems reasonable to reverse the factorial process; if x arrangements are now possible, for this to be measured as y, where $y! = x$. In the majority of cases x is not a true factorial, but the use of Γ function tables overcomes this. Thus "y" is the number, which if it could have a factorial, would have x as its factorial. This value y can be referred to as the "Equivalent Series," i.e. the problem is now at the same value of stimulus difficulty as would be presented for the first trial of a series of y objects. Thus after a first matching trial of four objects, none of which is correctly placed, $x = 9$, and the Equivalent Series is 3·31.

In one sense this is absurd, as 3·31 objects cannot be matched, but the transformation serves two useful functions. It restores the measure to a scale linearly related to the number of objects presented in the simple case (magnitude therefore can be simply appreciated). It also reduces skewedness so that simple statistical procedures are applicable.

The logarithmic transformation as used by Shannon is not as suitable as the Γ function in this case. With small probabilities of chance success, as in early trials with a large number of objects, the two measures are fairly closely related. But a logarithmic measure fails to differentiate between the condition of the problem being solved (E.S. = 0), and the condition of having all the necessary information to solve the problem (E.S. = 1). In terms of information this does not involve any confusion, but in terms of behaviour there may be, and often is, a difference between the two conditions. As the purpose of this experiment was to relate behaviour to information available, a single measure embracing both aspects of the problem was necessary.

First matching problem (the 8(1) *condition).*

The process of "calibrating" the stimulus difficulty for longer series of matching tasks is tedious. The measurement of the individual's performance merely involves calculating the number of arrangements possible taking into account the information gained, stage by stage. But an overall value is required for the task, and, strictly, this means taking each possible initial trial (or a representative of each class), counting the number of possible arrangements left, trying each of these (or again representatives) and seeing how many arrangements are left after the second trial and so on. Fortunately the task can be systematized. In the first place, the distribution of successes by chance on the initial trial is known. Secondly, if there are, say, four unsuccessful matchings in the first trial, then it does not matter how large the series is, the number of arrangements left is still the same: i.e. we may treat it as a series of four with no correct matches on the first trial. The computation thus is a matter of ascending from two incorrect matches on the first trial, to three, to four, etc. Later trials are more difficult, but can also be systematized.

This "calibration" has been performed for series of up to eight in length, eight being the value chosen for this experiment. With eight there are 8! possible arrangements (40320) on the first trial.

First trial frequency	*No. of arrangements for second trial*	*Equivalent Series*
1	0	0
28	1	1·00
112	2	2·00
630	9	3·31
2,464	44	4·39
7,420	265	5·45
14,832	1,854	6·50
14,833	14,833	7·52

Expected Mean E.S. after trial 1 = 6·487.

Following on with trial	2	Expected Mean E.S. is	4·656
” ” ” ”	3	” ” ” ”	2·508
” ” ” ”	4	” ” ” ”	0·810
” ” ” ”	5	” ” ” ”	0·128
” ” ” ”	6	” ” ” ”	0·006
” ” ” ”	7	” ” ” ”	0·000025
” ” ” ”	8	” ” ” ”	0·000

This provides the base line against which the average human performance can be measured. Any failures to use the information—either forgetting a correct match or repeating an incorrect match—meant that there were more arrangements left possible than there would have been had these errors not been introduced. This case of simple matching will be referred to as the 8 (1) condition of problem.

Second matching problem (the 4(2) *condition).*

A second matching problem using the same material was devised. Instead of matching eight objects to eight spaces, and being told after each trial which objects were correctly matched and which were not, eight objects were to be "placed," two in each of four cells. The information given, for each cell was whether there were 0, 1, or 2 objects correctly placed. If one was correctly placed and the other was not, the correct one was not specified, the information being that one was correct and one incorrect. The initial stimulus difficulty is less than in the 8(1) condition, as order within each cell is irrelevant. There are 2,520 initial possible arrangements, one sixteenth of 8!, the Equivalent Series being 6·65 (again referring to the equivalent as a simple matching problem). But the information gained from a trial may be conditional.

Consider the first trial:—

Correct	*1st Trial*		*Possible arrangements for 2nd trial*
AB	AE	—1 correct	either A with B, C, D or F; or E with B, C, D or F.
CD	CF	—1 correct	either C with A, B, D or E; or F with A, B, D or E.
EF	BD	—0 correct	Two from A, C, E and F.
GH	GH	—2 correct	G and H.

There are eight possible arrangements, but the A, E and the C, F alternatives may have to be remembered for subsequent trials. The first trial is straightforward, ranging from 1 arrangement completely correct, to 297 arrangements with no correct entry, and leaving 297 possible arrangements for the second trial. Problems of symmetry and of other forms of patterning make a systematic approach to the expected distribution somewhat laborious and the values given here may not be completely accurate.

Initial Equivalent Series	6·65
Expected Mean E.S. after trial 1	4·863
,, ,, ,, ,, ,, 2	2·623
,, ,, ,, ,, ,, 3	0·818
,, ,, ,, ,, 4	0·079
,, ,, ,, ,, ,, 5	0·0001
,, ,, ,, ,, ,, 6	0·000

This problem is referred to as the 4(2) condition.

Third matching problem (the 2(4) condition).

The third variation of the problem is on *a priori* grounds simpler still. The eight objects are to be allocated, 4 to each of 2 spaces. There are only 70 ways in which this can be done. One of these is completely right, one is completely wrong, giving the information that a straight interchange of the objects is all that is necessary. As in the 4(2) condition, information is given as to the number correct in each cell—it is bound in this case to be the same for both cells. This is the 2(4) condition, and treating the problem in the same way as the others, we have:—

Initial Equivalent Series	4·68
Expected Mean E.S. after trial 1	3·908
,, ,, ,, ,, ,, 2	3·095
,, ,, ,, ,, ,, 3	2·156
,, ,, ,, ,, ,, 4	1·200
,, ,, ,, ,, ,, 5	0·355
,, ,, ,, ,, ,, 6	0·110
,, ,, ,, ,, ,, 7	0·016
,, ,, ,, ,, ,, 8	0·000

In all conditions these values represent the calibration of the problems in terms of the average expected stimulus difficulty, trial by trial, throughout the performances. They represent the average of all possible performances by a machine which had perfect memory, but which otherwise acted purely at random. The performances of eighteen human subjects, measured in the same way, are compared with them.

Experimental material, instructions, and design.

The eight objects employed were:—

(1)	Block of wood ($2\frac{3}{4}$ in. × $1\frac{3}{4}$ in. × $\frac{7}{8}$ in.)	W
(2)	An empty safety match box	M
(3)	A piece of brass plate ($2\frac{1}{2}$ in. × 1 in. × $\frac{1}{8}$ in.)	P
(4)	A piece of brass tube—like a napkin ring ($1\frac{3}{8}$ in. diam. × $\frac{13}{16}$ in. ht.)	R
(5)	A meccano 1 inch gear wheel	G
(6)	A wood screw ($2\frac{1}{2}$ in. long)	S
(7)	A bolt with hexagonal head ($\frac{3}{4}$ in. long × $\frac{1}{2}$ in. head)	Bo
(8)	A small metal bracket (1 in. × $\frac{1}{2}$ in. × $\frac{1}{2}$ in. wide)	Bk

For the 8(1) condition a sheet of cardboard was divided into 8 spaces from left to right. For the 4(2) condition another sheet was marked out into quadrants, and for the 2(4) condition a sheet was divided into 2 cells, Left and Right. For an experimental session the subject sat at a table with one of the cardboard sheets on it, and the eight objects placed in a heap randomly beside it. In the 8(1) condition the instructions were, "Here are eight objects, and on the sheet of cardboard in front of you there are eight spaces. Each object has been allocated to a space at random, and your task is to find out which object goes where for them all. The procedure is this; after a trial (and a trial means all eight objects placed one in each space) I will place a card with the word "Correct" against each object which is in its correct cell. I will put nothing against an incorrectly placed object. You will then have half a minute to look at the display without manipulating it. At the end of that time I will take the cards away, shuffle the objects and return them to base and you will have another try. Except for the half minute for looking at the display there is no time limit."

Similar instructions were given in the 4(2) and the 2(4) conditions, except that they were told that 0, 1 or 2, or 0, 1, 2, 3 or 4 cards would be placed in appropriate cells, but that in the conditional cases they would be told not which objects were correct, but only the total correct in the cell.

Three random "correct" allocations were used and rotated between the conditions. The effects of order of trial (at least 48 hours interval between sessions was maintained) were minimized by a factorial design:—

i.e.	8(1)	4(2)	2(4)
Subject 1	1st	2nd	3rd
2	1st	3rd	2nd
3	2nd	1st	3rd
4	2nd	3rd	1st
5	3rd	1st	2nd
6	3rd	2nd	1st

Similarly for subjects 7–12 and 13–18.

For subjects 1–6 the correct orders were:—

8(1) Bk R M P G Bo W S

$$4(2)\quad \begin{array}{cc|cc} \text{G} & \text{W} & \text{P} & \text{Bk} \\ \hline \text{S} & \text{R} & \text{M} & \text{Bo} \end{array}$$

2(4) M P G S | W R Bo Bk

for subjects 7–12:—

8(1) G W P Bk S R M Bo

$$4(2)\quad \begin{array}{cc|cc} \text{M} & \text{P} & \text{G} & \text{S} \\ \hline \text{W} & \text{R} & \text{Bo} & \text{Bk} \end{array}$$

2(4) Bk R M P | G Bo W S

and for subjects 13–18:—

8(1) M P G S W R Bo Bk

$$4(2)\quad \begin{array}{cc|cc} \text{Bk} & \text{R} & \text{M} & \text{P} \\ \hline \text{G} & \text{Bo} & \text{W} & \text{S} \end{array}$$

2(4) G W P Bk | S R M Bo

The eighteen subjects were students, research workers and staff from the Psychological Laboratories at Cambridge and University College, London.

Thus the experiment is balanced for straightforward comparison between the three conditions. Record was made of where each object was placed in each trial. The number of possible arrangements left at the end of each trial was computed and transformed into the Equivalent Series.

II

RESULTS

The mean equivalent series, trial by trial, is shown for the three conditions in Table I. It will be seen that, compared with expectation, human performance in

TABLE I

PERFORMANCE OF SUBJECTS COMPARED WITH THEORETICAL PERFORMANCE
Equivalent series after trials

Trials	*Theoretical*	*Observed*
8(1) Condition:		
1	6·487	6·491
2	4·656	4·701
3	2·508	2·726
4	0·810	1·183
5	0·128	0·462
6	0·006	0·111
7	0·000025	0·056
8	0·000	0·000
4(2) Condition:		
1	4·863	4·785
2	2·623	3·244
3	0·818	1·525
4	0·079	0·667
5	0·0001	0·333
6	0·000	0·222
7		0·167
8		0·056
9		0·056
10		0·056
11		0·056
12		0·000
2(4) Condition:		
1	3·908	3·993
2	3·095	3·143
3	2·156	2·421
4	1·200	1·401
5	0·355	1·179
6	0·110	0·778
7	0·016	0·722
8	0·000	0·722
9		0·667
10		0·556
11		0·389
12		0·333
13		0·278
14		0·278
15		0·222
16		0·111

the 8(1) condition is reasonably good. But it is considerably poorer in the 4(2) and the 2(4) condition. Phenomenal difficulty in the latter two conditions would seem to outweigh stimulus difficulty to some extent. Nine subjects in the 8(1) condition behaved with perfect consistency—i.e. they never repeated an error nor forgot a correct match. Three subjects did so in the 4(2) condition and only 2 in the 2(4) condition. Similarly, as a crude indication of failure to take account of all information, we may consider those instances where complete information had been obtained (i.e. if full account was taken of all information, the next trial would be completely correct) but where the subject failed to respond and produced a wrong arrangement.

In the 8(1) condition 6 S's made 1 unnecessary trial
" " " " 1 S " 2 " trials

" " 4(2) " 4 S's " 1 " trial
" " " " 2 " " 3 " trials
" " " " 1 S " 4 " "
" " " " 1 " " 7 " "

" " 2(4) " 3 S's " 1 " trial
" " " " 1 S " 5 " trials
" " " " 4 S's " 6 " "
" " " " 2 " " 7 " "
" " " " 2 " " 11 " "

In the 2(4) condition two of the subjects failed to find a solution in 16 trials, at which point the experimental sessions were suspended.

The observations can be alternatively described in terms of the number of trials required for solution, compared in all cases with the number of trials expected on a hypothesis of perfect memory, but otherwise random behaviour.

TABLE II

OBSERVED AND EXPECTED NUMBER OF TRIALS FOR SOLUTION

	8(1) *Condition*		4(2) *Condition*		2(4) *Condition*	
	E.	O.	E.	O.	E.	O.
1st	·000	0	·007	0	·257	0
2nd	·110	0	·429	0	1·029	0
3rd	1·916	1	5·700	1	2·057	1
4th	6·975	5	10·454	6	4·038	4
5th	6·975	6	1·408	5	5·611	2
6th	1·916	4	·002	2	3·572	2
7th	·110	1	—	1	1·131	
8th	·000	1	—	2	0·285	
8+	—		—	1	—	9

An additional behavioural measure can be made in terms of survival of information. If we take the first trial, this has given some information, and any subsequent trial can be tested against it to see whether it is consistent, or infringes the information gained in trial 1. If the later trial is consistent, we have no real way of deciding whether this was deliberate or not, whether the subject had remembered and was using the information gained earlier, or whether the consistency had occurred by chance or was derived from information gained in intervening trials. If, however, the trial is not consistent, then we can assume either that the subject has forgotten the information, or for some reason is deliberately refraining from using it. Thus we can take, trial by trial, a measure of the survival of information. On the parallel with other survival distributions, we can assume this to be quasi-logarithmic in form, and take either the logarithmic decrement, or the half period as the measure of survival.

TABLE III

SURVIVAL TIME OF INFORMATION

8(1) Condition		*4(2) Condition*		*2(4) Condition*	
Trial	*½ period (in trials)*	*Trial*	*½ period*	*Trial*	*½ period*
1	6·81	1	2·41	1	1·71
2	4·96	2	1·65	2	1·33
2+	5·02	3	1·62	3	1·00
		3+	2·95	4–8	1·37
				8+	2·23
All	5·55		2·16		1·55

This agrees with the other quantitative data in indicating the relative phenomenal difficulty of the tasks. The differences between the trials may just be a normal memory effect, as shown in Ebbinghaus' work on serial learning. It may alternatively be a function of approach to the problem by some individuals who deliberately referred back to their first trial after subsequent trials, testing the observations gained against it and paying less attention to intervening trials. The improvement in consistency with later trials may be due to the fact that the problems were nearer solution, and most of the information was now systematized.

Behaviour in the first trial

The first trial differs from the succeeding trials in that the subject has no information to go on at all. He has been told that the correct matchings have been assigned by chance, and might therefore be expected to respond in an equally random manner. He does not in general do so.

In the 8(1) condition, the arrangement

1	2	3	4	5	6	7	8	
W	M	P	.	.	.	.	.	was given by 7 subjects
W	P	M	.	.	.	.	.	" " " 2 "
.	.	.	.	.	P	M	W	" " " 3 "

Thus 12 out of 18 subjects adopt some rudimentary form of a size or size and shape classification for their first trial.

Similarly in the 4(2) condition, in the first trial:—

W and M in same cell	12 subjects
R and G in same cell	9 "
P and Bk in same cell	8 "
S and Bo in same cell	8 "

5 subjects had all 4 pairs
6 " " 2 of the pairs
4 " " 1 pair
and 3 " " 0 pairs.

And further, in the 2(4) condition, 6 subjects divided the objects into W M P Bk on one side and R S G Bo on the other (a shape classification—square v. round). A further 5 had W M P on one side with some object other than Bk.

It will be seen how important it was to interchange the random "correct" layouts between the three conditions, as there obviously exist commonly preferred orders, and one of the random arrangements might be nearer to these than the others.

III

Discussion and Qualitative Observations

The first conclusion to be drawn from the observations is that phenomenal difficulty (measured from the behaviour) and stimulus difficulty (measured in the manner described) are not in agreement. It is suggested that this difference lies primarily in the nature of the information gained by the subject, varying as it does from the unequivocal "correct-incorrect" information in the 8(1) condition, through the simple conditional form—the either this is right or that is right—sometimes found in the 4(2) condition, to the more complex conditional form of one or two or three out of four are correct, found in the 2(4) case. Furthermore it is suggested that these differences force the subjects to adopt different methods of solution for the three tasks. A feature common to all three tasks is the attempt on the part of the subjects to impose methods of or aids to solution on to the task. The first of these has already been mentioned; the laying down of the material on the first trial in some sort of meaningful order. The advantages of this procedure are obvious: instead of having to remember the position of eight objects, all that is necessary is to remember the basis of the classification. It is the introduction of a generalization, not as a method of solution, but as a means of organizing, and consequently of simplifying the data. The majority of subjects adopted some form of classification, easily identifiable from the records, in all three tasks. In the 8(1) condition, many subjects adopted additionally a systematic approach to the solution, the best such system being "scanning." This is best illustrated by example.

Correct order	M	P	G	S	W	R	Bo	Bk	
1st trial ..	*W*	*M*	*P*	*R*	*G*	*S*	*Bk*	*Bo*	(size classification)
2nd trial ..	*Bo*	*W*	*M*	*P*	*R*	*G*	*S*	Bk	(moved one to right)
3rd trial ..	*S*	*Bo*	*W*	*M*	*P*	R	*G*	Bk	(continued)
4th trial ..	*G*	*S*	*Bo*	*W*	*M*	R	*P*	Bk	,,
5th trial ..	*P*	*G*	*S*	*Bo*	W	R	*M*	Bk	,,
6th trial ..	M	P	G	S	W	R	Bo	Bk	,,

Nine subjects used this method from the beginning, two others took it up after early trials. Some other systems adopted for part or all of the time were reversal of trial 1 order or interchange of adjacent pairs. In no case did there seem to be a complete absence of system of some form or other.

The combination of an initial classificatory layout, and systematic change between trials reduces the phenomenal complexity of the problem enormously, the only problem remaining is memory of the items correct—the others are taken care of. It is perhaps worth noting that two individuals, faced with a sudden large increase in the number correct on one trial, broke down on the system. One subject failed to remember an item which had been in its correct position for two trials, and the other failed to move one of the incorrect objects to its next scanning position. These may represent the effect of change from a set to avoid repeating errors to a set to retain the correct responses, but more evidence would have to be forthcoming for any definite conclusion.

The two other tasks, however, do not lend themselves to such systematic solution. In the 4(2) condition, several individuals attempted a systematic approach, even though it ignored some of the information.

As an example:—

Correct	M	P	G	S	W	R	Bo	Bk
Trial 1	*Bo*	*Bk*	*P*	S	*G*	*M*	*R*	*W*
" 2	*Bo*	*W*	*Bk*	S	*G*	*P*	*R*	*M*
" 3	*Bo*	M	*W*	S	*G*	*Bk*	*R*	*P*
" 4	P	M	*Bo*	S	*G*	W	*R*	Bk
" 5	P	M	*R*	S	*Bo*	W	*G*	Bk
" 6	P	M	G	S	R	W	Bo	Bk

In this case the information from the first trial (i.e. that except for either P or S, none of the objects was in its correct position) was not wholly employed. Instead, Bo, S, G and R were left in position and the others were scanned.

This may have an advantage, in cases where several cells have no correct entry, in making any information gained unconditional—as appears in the example given, at trial 3, where M is certainly placed. But if there are cells initially containing one correct object, certainty is not achieved. In the example, trial 2 gives one correct entry in cell 2, as had been the case with trial 1. Although on a probability basis it is likely that this is S, it is not certain, as the same information would have been given were P and Bk the correct objects for that cell. A system like this, or some of the other less definite forms observable in the records implies a surrender of information, reducing the chance of early success in an endeavour to reduce the memory load of the task. Records of eight subjects indicate similar non-acceptance of information in keeping with some form of system.

Loss of information is even more marked in the third 2(4) condition, though it is less easy to relate this to any systematic approach. Rather than producing a system, and acting on it, individuals seem to make a hypothesis as to which objects are correct, and to test this hypothesis, and modify it trial by trial. This involves reference back to earlier trials. In a few cases trials appear to be referred to the first trial only. An example

Correct order	G	W	P	Bk	S	R	M	Bo	*Trial consistent with trials*
Trial 1	*M*	W	P	Bk	S	R	*G*	Bo	
" 2	*M*	W	*R*	Bk	S	*P*	*G*	Bo	1
" 3	*M*	W	P	*Bo*	S	R	*G*	*Bk*	1, 2
" 4	*M*	*S*	P	Bk	*W*	R	*G*	Bo	1, 2, 3
" 5	*M*	*Bo*	P	Bk	S	R	*G*	*W*	1, 2
" 6	*M*	*Bo*	P	Bk	S	R	*G*	*W*	1, 2 (repetition trial 5)
" 7	G	*M*	P	Bk	S	R	*W*	Bo	1, 2, 3
" 8	*Bo*	*M*	P	Bk	S	R	*W*	*G*	1, 2, 7 (rep. of trials 5 and 6)
" 9	*R*	*M*	P	Bk	S	*W*	*G*	Bo	1, 3, 7
" 10	*R*	*M*	P	Bk	S	*W*	*G*	Bo	1, 3, 7 (rep. trial 9)
" 11	*M*	W	P	*S*	*Bk*	R	*G*	Bo	1, 2, 4, 5, 6, 8, 9, 10
" 12	*M*	*S*	P	Bk	*W*	R	*G*	Bo	1, 2, 3, 7 (rep. of trial 4)
" 13	*Bo*	W	P	Bk	S	R	M	*G*	1, 2, 4, 9, 10, 11, 12
" 14	*S*	W	P	Bk	*G*	R	M	Bo	1, 2, 3, 5, 6, 8, 9, 10, 13
" 15	*R*	W	P	Bk	S	*G*	M	Bo	1, 3, 4, 5, 6, 8, 11, 12, 13, 14
" 16	G	W	P	Bk	S	R	M	Bo	Consistent with all

Every trial is consistent with the first trial. The second trial also has some effect. But every move was just another attempt at solving the problem as left at the end of trial 1. This can be seen from the repetitions of trials—particularly the two instances of immediate repetition. It is worth noting that, had the information been accepted, the problem would have been solved on the fifth trial. It is also worth noting that there are only 16 possible arrangements consistent with trial 1, and it is somewhat surprising, given this consistency with the first trial, that the solution was not reached earlier as a matter of chance.

Another method of attempted solution lay in referring trials back to their immediate predecessors, neglecting information obtained earlier. As an example:—

									Trial consistent with trials
Correct order ..	M	P	G	S	W	R	Bo	Bk	
Trial 1	M	P	*W*	*Bk*	*S*	R	Bo	*G*	
" 2	M	*W*	*Bo*	S	*P*	R	*G*	Bk	1
" 3	*W*	*Bo*	G	*Bk*	*M*	R	*P*	*S*	1, 2
" 4	M	*Bo*	G	*R*	W	*P*	*S*	Bk	2
" 5	M	*Bo*	G	*Bk*	W	R	*P*	*S*	1, 2
" 6	M	*W*	*R*	*Bk*	*P*	*S*	Bo	*G*	2, 4, 5
" 7	*R*	*Bo*	G	S	W	*M*	*P*	Bk	2, 5, 6
" 8	*W*	P	*R*	*Bo*	*M*	*S*	*G*	Bk	1, 2, 4, 7
" 9	M	*Bo*	G	S	W	R	*P*	Bk	6, 8
" 10	M	*Bo*	G	*R*	W	*P*	*S*	Bk	2, 9 (repetition trial 4)
" 11	*Bo*	P	G	S	W	R	*M*	Bk	2, 4, 5, 9, 10
" 12	M	P	*Bo*	S	W	R	*G*	Bk	1, 3, 4, 5, 6, 9, 10, 11
" 13	M	P	G	S	W	R	Bo	Bk	all

In this case, except for trials 4 and 5, all the trials are consistent with the trial immediately preceding, and several are consistent with the two immediately preceding trials, but on the whole information gained earlier than that is ignored. This procedure stands in contra-distinction to the example given immediately above, and yet there is one important feature in common. These individuals (like almost all of the subjects) either could not or would not act upon all the information. The difference lies in their selection of information.

In a few instances there were examples of assumptions made which were erroneous. Some of these cases bear striking resemblance to fixated behaviour more usually mentioned in connection with experimental studies of conflict. As an example:—

Correct order ..	Bk	R	M	P	G	Bo	W	S
Trial 1	*G*	R	*W*	P	*Bk*	Bo	*M*	S
" 2	*Bo*	*S*	*W*	P	G	*R*	*Bk*	*M*
" 3	*G*	R	*S*	P	*Bk*	Bo	W	*M*

At this stage the subject decided that G and R should be placed in the left hand cell. For the next eleven trials these two objects were taken up first and placed confidently on the left, the subject then studying the remaining six objects. After the fourteenth trial this broke down, and G was correctly transferred to the right hand cell. Repeatedly during the eleven trials information was presented which indicated that both G and R could not be on the left, but it was ignored.

If an analogy with another type of problem is possible, these three examples are like three different attempts at a jigsaw puzzle. The first is like taking one piece and trying the other pieces against it; the second like taking one piece and trying another against it, taking a third piece and trying it against the second, and so on; whereas the third example is rather like deciding that "this brown piece must be part of the horse's head."

Another interesting feature of the observations on this condition is that behaviour, with respect to the information of correct or incorrect does not correspond to the actual value of the information given. As the objects are to be placed in either of two cells, it follows that the information that an object is incorrectly placed is equal in value to the information that it is correctly placed—merely requiring the subject to reverse its position. Information that none of the objects are in their appropriate cells is complete information. Three subjects reached this position. Two failed to act upon the information, although a subsequent report from one subject implied that this was a memory failure rather than a failure to appreciate the value of the information. The third subject responded correctly, after a long pause, and with the observation "Would I be naïve if I thought that if I reversed them they would all be right?"

Similarly, information that one object in each cell was correctly placed is equal in value to information that three objects are correctly placed in each cell. Both leave only sixteen possible arrangements, whereas the condition of two right in each cell leaves 36 arrangements. Behaviour was not consistent with this, however, for if we take survival times of information from trials with these "success" scores we have:

1 correct each cell	½ period of survival of information	0·72 trials
2 " " "	" " " " " "	1·09 "
3 " " "	" " " " " "	4·84 "

The phenomenal difficulty of the task after a one correct trial is thus much greater than after a three correct trial even though the stimulus difficulty is the same. This may mean that the value of "correct" is increased over "incorrect" by the emotional implications of the terms. Or it could merely be a carry over from more normal problem situations (as in the two other experimental conditions) where this equality of value in terms of stimulus difficulty does not hold. Or as a third possibility it could be that the subjects' set was towards the placing of four correct objects into a cell, rather than the placing of each object into one of the two cells, and that the complementary nature of the two cells, being one stage removed in the formulation of the problem, did not affect their behaviour.

The nature of trial and error problem solving.

The nature of the trial-and-error problem has already been discussed, with its emphasis on the progressive reduction of number of possible modes of behaviour during the course of solution. Ideally, the aim of the solver—human, animal or mechanical—is to make this reduction as efficiently as possible. The experimental results indicate that the human subject, under some conditions, does not show mechanical efficiency. Some speculation is possible as to the nature of this distinction and its relation to efficiency in the psychological sense of use of the subject's own perceptual, memory and cognitive abilities. The number of objects used in these experiments was partly chosen because of its divisibility into the three experimental conditions, and also partly as it is about the normal maximum for immediate memory span for position. Twelve objects would have suited the experimental design, even more so than eight, but even trial-to-trial consistency would have been problematic, in the face of the immediate memory task involved. Even with eight objects it is doubtful whether the normal individual's ability to remember would, by itself, have enabled him to solve any of the problems. Thus memory aids, or a judgment of the important information to be remembered are a necessity. The most customary memory aid, notes on paper, was denied. The initial classification of objects, observed in all experimental conditions, and the systems such as scanning, in the first condition, are very good substitutes. The simple matching problem is so greatly reduced in phenomenal difficulty by them that it almost ceases to be a problem, and is certainly the easiest of the three. The memory load is reduced to immediate memory of correct and incorrect and direction of scanning for each trial only, without any necessity to carry information over longer periods by sheer memory. The two generalizations, of order and of movement together take up all the information and reduce it in its psychologically quantitative size to an amount easily coped with by the individual. The same essential feature is observed in the other conditions, only in these cases the reduction has to be made by the selection of information—after the initial memory aid of classification of objects. This selection takes place in different ways, mainly by selecting some of the objects and ignoring the others, or by selecting some of the trials and ignoring others, or by selecting some inferences from trials and ignoring others. In real life problem solving situations, in addition to these methods of selection (which may be persistent behavioural characteristics of the individuals) there are usually preformed evaluations of the value of different pieces of information, evaluations carried over from previous experience or training, etc. This only differs in the experimental situation in that the experiment has an initial trial, which can be considered as effectively absent in the real life situation.

It could be paralleled experimentally, by the use of material in which there were inherent clues as to the correct position; by instructing the subjects that there was some sense in the arrangement not just a random allocation. Some earlier experiments showed that inherent clues produced a greater chance of early success, but if this were not attained they interfered with any later solution. These experiments were performed using the simple matching problem only (like the 8(1) condition); it would be of interest to see whether, in the more conditional cases the inherent clues took greater priority of attention.

It is tempting to speculate on the relation of intelligence to these describable aspects of problem solving. For a functional definition, intelligence is bound to be concerned with ability to solve problems, but the working definitions on which most measurements of intelligence are based restrict it either to an appreciation of the formal structure of problems, or the ability to comprehend and manipulate all the information for a series of simple problems. The choice of generalization, or the process of selection and rejection of information are very rarely demonstrated in intelligence tests, and yet it may be that these are far more important in the normal use of intelligence; the production of a tentative "solution" to problems, the formal structure of which escapes comprehension.

The experiment described does not throw any light on this. Its design, balanced with respect to the three conditions, makes it difficult to compare subjects. In addition, the variation in chance success in the first trial, assumed to be balanced as far as conditions are concerned by using eighteen subjects and rotating the experimentally "correct" orders between the conditions, is bound to lower any correlation of subject's performance between the three conditions. An experiment specifically aimed at this end is perfectly feasible. It would presumably require more subjects, and some standard system of constructing the "correct" order from the subject's first trial devised. It ought then to be seen whether there was any correlation in performance, and perhaps more important, any persistence in the qualitative methods of solution adopted. The observations would have to be validated against an external criterion of degree of success in coping with real life problems, if anything of the nature of intelligence is to be associated with the performance.

The writer wishes to thank his colleagues in the Psychological Laboratories at Cambridge University and University College, London, for their willingness to act as subjects, and their helpful discussion of the observations.

REFERENCE

Shannon, C. E. (1948). A Mathematical Theory of Communication. *The Bell System Technical Journal*, **27**, 379–423.

observation "Would I be naïve if I thought that if I reversed them they would all be right?"

Similarly, information that one object in each cell was correctly placed is equal in value to information that three objects are correctly placed in each cell. Both leave only sixteen possible arrangements, whereas the condition of two right in each cell leaves 36 arrangements. Behaviour was not consistent with this, however, for if we take survival times of information from trials with these "success" scores we have:

1 correct each cell ½ period of survival of information 0·72 trials
2 " " " " " " " " " 1·09 "
3 " " " " " " " " " 4·84 "

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BOOK REVIEW

Handbook of Experimental Psychology. Ed. S. S. Stevens. London: Chapman and Hall, 1951. Pp. xi + 1,436. 618 illus. £6.

Julian Huxley, in his "Essays of a Biologist," may have first set down the view that "The biologist cannot fail to be impressed by the fact that his science to-day is . . . in the position which Chemistry and Physics occupied a century ago. It is beginning to reach down from observation to experimental analysis, and from experimental analysis to grasp of principle." If there is no *a priori* case for the same optimistic view of psychology, the recent success of a few less ambitious books at least makes the position arguable, and the logic for a compendious survey of the facts and an examination of the theories proposed to account for them should not be too difficult to find. Such a handbook must, however, justify itself not only in its occasion but also by its scope, having more to contribute than a collection of reviews from the Psychological Bulletin and yet lacking the partisan selection of material which goes with a narrower intention. It was the achievement of Murchison's 1934 handbook, now long out of print, to provide the research worker with a mine of information and at the same time, by virtue of the balance struck between the persuasions of its various contributors, to give a much used, if seldom quoted, baseline for the research of the next decade. We should not, then, be surprised if its successor fell short of this high standard; that the present volume falls so far below is disappointing. With some notable exceptions where the writer has been aided by the quality of his subject, the limitations of space conspire with the intention to include all normal experimental psychology, and much else besides, to give the reader an impression not so much of superficiality as of an uneasy fluctuation on the part of the contributors between the lexicographic completeness of a Catalogue of Ships and the condensed obscurity of the Funeral Oration. The effect is most marked in the section devoted to learning, which should form the core of such a book as this, and while it is a matter of bias whether one would sacrifice some of the physiological detail or the four chapters given over to applied psychology, it is clear that a more radical operation than Stevens ever contemplated is needed if the experimental psychologist is to come to terms with the economics of publishing.

The literature on the special senses lends itself more readily than any other to a review of the kind attempted in this book, and so it is that the papers of Judd, Bartley and Graham on the physics, physiology and psychology of vision, and an impressive parallel contribution mainly from the Psycho-Acoustic Laboratory at Harvard contrive to be both concise and informative. Of particular interest is the review by von Békésy and Rosenblith of the senior author's work on the "Mechanical Properties of the Ear" and a chapter by Licklider and G. A. Miller on the "Perception of Speech." Bartley's summary of research since 1934 serves to correct the more speculative account given by Hecht in the previous handbook, though it is curious that neither of his sections on visual acuity gives any reference to Marshall and Talbot's contribution to this problem. Graham has an admirably clear account of the literature on the perception of space and movement, together with a factual review of the Köhler after-effect.

This seems to be the best place to comment on Stevens' introductory paper, "Mathematics, Measurement and Psychophysics." As a statement of his own position it is less adequate than his previous writings, and his dismissal of Campbell's arguments perhaps a little cavalier. A critical analysis of topological concepts, to which he gives passing reference, and some account of the sociometric techniques of Guttman and Coombs would have added to the value of this chapter.

While the post-war years have witnessed an increasing scrutiny of some of the fundamental ideas of learning theory, the temper of the previous decade was, with the few exceptions when *expectancy* and *response* theories made contact, to accumulate facts and neglect their interpretation. The renewed interest in secondary reinforcement, the goal gradients, operant conditioning and so on is of too recent an origin to have influenced the character of the reviews in this book, and so it is something of a mixed blessing that the weight of evidence at present favours an S-R rather than any other theory: we are spared the wordy discourse of the Gestalt school only to find a too ready indulgence in the anomalies of Hull's position. We miss the Lashleian eye.

There are two chapters in the section on learning which, if we accept the limitations imposed upon them by space, are well worth reading. Spence gives a very fair account of Hull, Tolman, Skinner and Guthrie, and of the issues between them. Brogdens's review of the conditions affecting the acquisition and transfer of CR is excellent, though this has been achieved at a considerable price, discrimination- and maze-learning receiving only the most summary treatment. Human learning is an untidy subject, and Hovland, by devoting a page or two to each topic *seriatim*, does not warm to his subject until he gets to reminiscence and transfer. The English reader will be disappointed to find little or nothing on remembering, recognition and the effects of set. Of the other chapters in this section, Hilgard on "Method" and C. T. Morgan on "The Psychophysiology of Learning" are adequate, while the papers of Leeper on "Cognitive Processes" and G. A. Miller on "Speech and Language" are less impressive. It will be sufficient if we comment on a chapter of similar quality by N. E. Miller on "Learnable Drives and Rewards" which follows upon Beach's interesting review of studies of reproductive behaviour in a section on motivation. The importance of secondary reinforcement to Hull's theory is such that the inclusion of a chapter devoted to the subject needs no justification. (Indeed the conception of organizing a series of papers on learning by subject rather than by species is one that might have commended itself to the Editor.) Miller's contribution, however, strikes us as incoherent in arrangement and not always convincing in its argument.

We are in no way qualified to comment in detail on the remaining two sections of this handbook. Brink and Ruch contribute two chapters each on neuronic and cerebral mechanisms respectively, and Dempsey completes the physiological section with a paper on "Homeostasis." The other, entitled "Growth and Development," contains an interesting review by Sperry of research on neurogenesis. C. S. Hall's survey of genetics provides the uninitiated with some useful comments on method in this field together with a short bibliography of some of the literature. Nissen's paper on interspecific comparison contains many references, omitted by the other contributors, which are relevant to motivation and learning, but his argument sometimes gets lost in the midst of these digressions.

In conclusion we must note the inadequacy of the subject index to this handbook. For instance, a long and useful footnote by Hovland on the learning of meaningful material (p. 622), and his paragraph on changed set (p. 677), are not referred to, and the tendency of different authors to use different terms, such as negative adaptation and habituation (pp. 364–366), is not always allowed for by cross references.

The present *Handbook of Experimental Psychology* is, in the jargon of the book reviewers, likely to remain the standard work for a number of years. While this assumption underlies the tenor of the present account, our criticisms should not be allowed to conceal the fact that it will be of considerable use to the research worker and, perhaps, to the less dyspeptic among the undergraduates. Nevertheless, all those familiar with "Murchison" will come with mixed feelings upon the enigmatic entry in the Subject Index—"It, antecedent of, removal by Editor, 293."

P. H. R. J.

ERRATUM.—In: Szafran, J. Changes with age and with exclusion of vision in performance at an aiming task, the concluding paragraphs on page 112 from "When the distributions of all time measurements . . ." to ". . . developed by Kendale (1948) and Whitfield (1947)." on page 113 should be read as a legend to Table I on page 113. The paragraph immediately above Table II on page 114 beginning with "The differences in the values . . ." should be read as a legend to Table II.

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